

Book of Abstracts



EENPS 2018 Conference

Comenius University in Bratislava
Department of Logic and Methodology of Science
Faculty of Arts
Bratislava, Slovakia
June 20-22, 2018

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EENPS 2018 Bratislava Conference Program

June 20 – Wednesday

08:30-09:50	Registration (G127)
10:00-10:15	Conference Opening
10:15-11:15	Vincenzo Crupi: Rationality and Reasoning Research: A Guide for the Perplexed Keynote lecture / Chair: Lilia Gurova (G127)
11:15-13:30	Lunch break

	A: General Philosophy of Science (G127) Chair: Jaana Eigi	B: Philosophy of Natural Sciences (G140) Chair: Özlem Yılmaz	C: Philosophy of Cognitive and Behavioral Sciences (G236) Chair: Miloš Kosterec
13:30-14:00	Gustavo Cevolani: Probability, truthlikeness, and two paradoxes of rational belief	María Ferreira Ruiz & Mariana Córdoba: Biological information – what was the problem again?	Nina A. Atanasova: Virtual Morris Water Maze: The Independent Life of an Experimental System
14:00-14:30	Borut Trpin: A Problem for Jeffrey Conditionalizers	Atoosa Kasirzadeh: The explanatory role of cellular automata models in biology	Blazej Skrzypulec: Nonclassical Mereology of Odours

14:30-15:00	Coffee break (Atrium – Faculty of Arts)
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	A: General Philosophy of Science (G127) Chair: Lilia Gurova	D: Philosophy of Social Sciences (G140) Chair: Juraj Halas
15:00-15:30	Maria Panagiotatou: ‘Local realism’ and the misconceptions about scientific realism	Magdalena Małecka: The normative theory of decision making in economics. A philosophical evaluation.
15:30-16:00	Ladislav Kvasz: Instrumental Realism	Monika Foltyń-Zarychta: Valuing non-market goods for intergenerational investments – explicit moral judgements in willingness to pay and willingness to accept compensation

16:00-16:30	Coffee break (Atrium – Faculty of Arts)
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	A: General Philosophy of Science (G127) Chair: Daniel Kostić	E: History, Philosophy and Social Studies of Science (G140) Chair: Daniela Glavaničová
16:30-17:00	Demetris Portides: Abstraction in Scientific Modeling	Hakob Barseghyan, Gregory Rupik & Patrick Fraser: Re-integrating HPS: Scientonomy as a Missing Link
17:00-17:30	Martin Zach: Scientific representation: Resituating the similarity account	Eden Smith: Examining the Structured Uses of Concepts as Tools: Converging Insights

17:30-19:30	Open Air Reception (Atrium – Faculty of Arts)
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June 21 – Thursday

09:30-10:30 Barbara Osimani: Games in Science: Reliability, Reproducibility, and Reputation
Keynote lecture / Chair: Lukáš Bielik (G127)

10:30-11:00 Coffee break (Atrium – Faculty of Arts)

	B: Philosophy of Natural Sciences (G127) Chair: Juraj Halas	C: Philosophy of Cognitive and Behavioral Sciences (G140) Chair: Nina A. Atanasova	E: History, Philosophy and Social Studies of Science (G236) Chair: Daniela Glavaničová
11:00-11:30	Cristian Ariel López & Manuel Herrera Aros: Getting physical possibility straight: what makes an event physically possible?	Mario Günther: Interventionist Mental Causation and the Methods of Cognitive Neuroscience	Jaana Eigi: Philosophy of Science and Inductive Risk
11:30-12:00	Manuel Herrera Aros: Physical Causation in General Relativity	Marek Pokropski: Phenomenology and multilevel mechanistic explanations in cognitive sciences	Francesca Biagioli & Flavia Padovani: From Mathematical to Physical Coordination and Back. Why Mathematical Coordination Can Be More Entangled than it Looks Like.

12:00-14:00 Lunch break

	A: General Philosophy of Science (G127) Chair: Borut Trpin	B: Philosophy of Natural Sciences (G140) Chair: Lilia Gurova	A: General Philosophy of Science (G236) Chair: Miloš Kosterec
14:00-14:30	Luca Tambolo: What, if anything, does counterfactual history teach us about the contingency of science?	Özlem Yılmaz: What is 'Individual Plant'?	Vladimir Drekalović: About the (im)perfection of the best mathematical explanations in science - the cicada case versus the Königsberg bridge case
14:30-15:00	Matthew Baxendale: Levels of Organization in Scientific Practice: Epistemic Tools for System Building	Guglielmo Militello: Functional Integration in the Endosymbiotic Origin of Mitochondria	Mihail – Petrișor Ivan: Poincaré's one conventionalism

15:00-15:30 Coffee break (Atrium – Faculty of Arts)

	A: General Philosophy of Science (G127) Chair: Duško Prelević	B: Philosophy of Natural Sciences (G140) Chair: Manuel Herrera Aros
15:30-16:00	Daniel Kostić: Minimal structure explanations, scientific	Sebastian Fortin & Jesús Alberto Jaimes Arriaga: The problem of the 3N dimensions in Quantum

	understanding and explanatory depth	Mechanics: a chemical approach
16:00-16:30	Richard David-Rus: Defending non-explanatory understanding: the case of possible explanations	Alfio Zambon & Fiorela Alassia: About the limits of the chemical periodic system

16:30-18:30 EENPS meeting (G127)

June 22 – Friday

	A: General Philosophy of Science (G127) Chair: Richard David-Rus	D: Philosophy of Social Sciences (G140) Chair: Monika Foltyn-Zarychta	E: History, Philosophy and Social Studies of Science (G241) Chair: Eden Smith
09:30-10:00	Stefan Petkov: In defence of veridicality, pragmatic truth and scientific understanding	Akos Sivado: Thinking through Kinds: the Ontological Turn meets Interpretive Social Science	Michele Luchetti: Constituting frequency changes in genetic populations via approximation and stability: Understanding the role of the Hardy-Weinberg principle through its epistemic history
10:00-10:30	Duško Prelević: Kuhn's Incommensurability Thesis: Good Examples Still to Be Found	Mariusz Maziarz: The 'why' and 'how' of causal inferences in economics	Mariana Córdoba & María Ferreira Ruiz: When science becomes a problem. Identity, biology and politics
10:30-11:00		Radim Chvaja: Memetics as Pseudoscience	Elena Sinelnikova: Philosophy of Science in Russia: the St. Petersburg Philosophical Society (1897-1923)

11:00-11:30 Coffee break

	B: Philosophy of Natural Sciences (G127) Chair: Martin Zach	A: General Philosophy of Science (G140) Chair: Lukáš Bielik
11:30-12:00	Vlasta Sikimic: Argumentative structures in biology: a study of pathogen discoveries	Kertész Gergely: Doubting the argument from constraining effects against causal closure
12:00-12:30	Damian Luty: Non-individuals and Structural Reconceptualization of Objects in Spacetime Structuralism	Anton Donchev & Mila Marinova: The Influence of Prior Probabilities on Judgments about Explanatory Power

12:30 Conference Closing (G127)

Keynote Speakers

Rationality and Reasoning Research: A Guide for the Perplexed

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Diagnoses of irrationality often arise in the empirical investigation of human reasoning. How can such diagnoses be disputed and assessed? I will articulate a principled classification of different cases relying on a view of experimental work from a philosophy of science perspective. We will then see that much fruitful research done with classical experimental paradigms was triggered by normative concerns and concurrently fostered scientific progress in properly psychological terms. The framework outlined provides new insight into many cornerstone examples, including Wason's selection task, the conjunction fallacy, so-called pseudodiagnosticity, and more besides. My conclusion will be that normative considerations retain a constructive role for the psychology of reasoning — contrary to recent complaints in the literature — but not the one that "normativist" cognitive scientists (including prominent Bayesians) have often assumed. In particular, the approach I propose does not blur the is-ought distinction.

Games in Science: Reliability, Reproducibility, and Reputation

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Contributions in the philosophy of science pointing to the social character of the scientific enterprise (Goldman, 1999; Kitcher 2003; Solomon, 2001; Douglas, 2000, Steel and Whyte 2012) illustrate how social dimensions of science are (intrinsically) intertwined with its standard epistemological goals. This is all the more evident in domains that have practical consequences for the citizen and society at large, such as health and the environment. In a society where science is also financed by public investments, and health and environment are constitutionally protected goods, individuals, interest groups, industry, as well as governmental agencies and institutions obviously create a complex network of dynamic, strategic interactions.

This complex web of interactions may characterize each scientific domain distinctively. I focus in the present talk on medicine and the pharmaceutical industry and analyse how issues of reliability, reproducibility, and reputation constrain the games played by the different actors involved, and particularly how such strategic dimensions are embedded in evidential requirements and methodological standards.

Contributed abstracts

A. General Philosophy of Science

Levels of Organisation in Scientific Practice: Epistemic Tools for System Building

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Until recently the concept of ‘levels of organisation’ was synonymous with the Layer Cake Model (LCM); (Oppenheim and Putnam, 1958). The LCM posited a global account of levels according to which every object of scientific inquiry could be placed into a single hierarchical structure ranging from fundamental particles at the bottom, up through atoms, molecules, cells, multi-cellular organisms and so on. The LCM also organised scientific inquiry into distinct disciplines that latch onto an individual level of the model e.g., physics, chemistry, biology, social sciences etc. Whilst this is arguably a well-known organisational structure for science it has long since been widely rejected as playing any significant role in contemporary scientific practice or reflecting the organisation of our scientific inquiries (Brooks, 2016; Feibleman, 1954; Guttman, 1976; Potochnik and McGill, 2012; Waters, 2008). Recently, a growing discussion in philosophy of science has precisely concerned whether ‘levels of organisation’ has outlived its utility as a concept in both scientific practice and the philosophical analysis of it (DiFrisco, 2016; Eronen, 2013, 2015) or whether the concept can still serve to illustrate an important aspect of scientific practice in a very different form to the LCM (Bechtel, 2017; Brooks, 2017; Craver, 2015; Kaiser, 2015).

In this paper I provide a new pluralistic analysis of levels of organisation with the aim of showing the concept’s centrality to many forms of scientific practice and a fruitful component in our philosophical analysis of it. My account proposes that levels of organisation are collections of purpose-relative vertical and horizontal principles deployed as representative tools in the conceptualisation of the target system of inquiry. Any pluralistic analysis of a scientific concept must negotiate a balance between being flexible enough to accommodate a wide-variety of different deployments of the concept in scientific practice, yet finding some minimally unifying link between those usages such that the concept maintains currency as a tool of analysis.

The unifying link in my own analysis is provided by the role that the levels play in system conceptualisation. Specifically, I argue that they are central to defining a target system of inquiry through the deployment of different vertical organizational principles; principles that differentiate parts from wholes. To illustrate this point I take a case study from three different system types that employ differing vertical principles: an aggregative system (Human Microbiome Project); a component system (Insulin Action Mechanism); and an integrated system (MAPK Cascade). From a philosophical perspective, horizontal principles can help us to understand the integration of differently acquired datasets into newly conceptualised models. I demonstrate this point by zooming in on the Insulin Action Mechanism and illustrate how unpacking these organisational principles can help to analyse the integration of different pathways investigated using different methodological techniques: primarily in vitro and in silico experimentation.

Identifying the specific combination of vertical and horizontal principles contributes to an analysis of the integration of different methodological techniques for different purposes. In the case of the Insulin Action Mechanism, to enhance treatment options for diseases that are a result of the malfunctioning of the mechanism (e.g., diabetes) by connecting information gathered through computation modelling of complex protein interactions at the molecular level to well-established information regarding clinical diagnosis and intervention at the organ/tissue level. The analysis I provide is fully epistemic – levels are tools for representing systems – and eschews metaphysical commitments regarding levels of organisation.

Whilst this approach easily avoids the problems associated with the LCM, it entails a burden to show how the concept remains fruitful for scientists and philosophers alike. Accordingly, the

demonstration of these organization principles in action and the identification of the purpose-relativity of their deployment provides a strong argument against claims that levels of organisation are neither scientifically nor philosophically useful; highlighting their role in multi-perspective approaches to modelling complex phenomena through the organisation and integration of data gathered through a variety of techniques.

References

- Bechtel, W. (2017): Using the hierarchy of biological ontologies to identify mechanisms in flat networks. *Biol Philos* 32, 627–649. <https://doi.org/10.1007/s10539-017-9579-x>
- Brooks, D. (2016): *The Levels of Organisation Concept in Biology*. Universität Bielefeld, Bielefeld.
- Brooks, D.S. (2017): In Defense of Levels: Layer Cakes and Guilt by Association. *Biol Theory* 12, 142–156. <https://doi.org/10.1007/s13752-017-0272-8>
- Craver, C.F. (2015): Levels. *Open MIND* 8, 1–26. <https://doi.org/10.15502/9783958570498>
- DiFrisco, J. (2016): Time Scales and Levels of Organization. *Erkenn* 1–24. <https://doi.org/10.1007/s10670-016-9844-4>
- Eronen, M.I. (2015): Levels of Organization: A Deflationary Account. *Biology and Philosophy* 30, 39–58.
- Eronen, M.I. (2013): No Levels, No Problems: Downward Causation in Neuroscience. *Philosophy of Science* 80, 1042–1052.
- Feibleman, J.K. (1954): Theory of Integrative Levels. *The British Journal for the Philosophy of Science* 5, 59–66.
- Guttman, B. (1976): Is Levels of Organisation a Useful Biological Concept?. *BioScience* 26, 112–113.
- Kaiser, M.I. (2015): *Reductive explanation in the biological sciences*. Springer, Cham.
- Oppenheim, P., Putnam, H. (1958): The Unity of Science as a Working Hypothesis. In: Feigl, H., Scriven, M., Maxwell, G. (eds.): *Minnesota Studies in the Philosophy of Science*. Minnesota University Press, Minneapolis, pp. 3–36.
- Potochnik, A., McGill, B. (2012): The Limits of Hierarchical Organization. *Philosophy of Science* 79, 120–140.
- Waters, C.K. (2008): Beyond theoretical reduction and layer cake anti-reduction. In: Ruse, M. (ed.): *The Oxford Handbook of Philosophy of Biology*. Oxford University Press, Oxford, pp. 238–262.

Probability, truthlikeness, and two paradoxes of rational belief

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Providing a theory of rational belief or acceptance in the face of uncertainty remains a crucial issue for current research in (formal) epistemology and philosophy of science. Two main accounts are currently on offer. On the one hand, we have a qualitative account of “plain” or “full” belief: for some consistent and logically closed set A of propositions representing the belief of a rational agent, the agent believes proposition h just in case h is in A , rejects h if $\neg h$ is in A , and suspends the judgment on h if neither h nor $\neg h$ are in A . On the other hand, we have a Bayesian, quantitative account of degrees of belief, which represents the subjective credences of our agent as probabilities. Both accounts, the qualitative and the quantitative-probabilistic one, have merits and drawbacks. In particular, the former is simple and compelling, but doesn’t deal well with uncertainty; while the latter does provide an account of uncertain belief, but not one of plain rational belief.

Attempts to combine the two accounts just outlined face well-known difficulties. One popular way to go is the so-called Lockean thesis: a rational agent should (fully) believe or accept proposition h

just in case his probabilistic degree of belief in h is greater than some suitably chosen threshold. Unfortunately, this idea clashes against the well-known Lottery and Preface paradoxes which, taken together, show that high probability is neither sufficient nor necessary for rational belief (Kyburg 1961; Makinson 1965; Foley 1992). Thus, one is seemingly left with two unpalatable options: either renouncing a proper treatment of uncertain belief in favor of a simple account of full belief, or giving up any belief-talk and remain content solely with an account of probabilistic credences.

In this paper, I propose a novel way out of this impasse. I assume that rational belief aims at approaching truth about the domain of inquiry, and that a rational agent should tentatively believe the strongest proposition h which is estimated as sufficiently close to the truth given the available evidence (Oddie 2014; Niiniluoto 1987). I call this idea Carneades' thesis on belief, after the Hellenistic philosopher who apparently first defended it within a coherently fallibilistic epistemology (Niiniluoto 1987). I provide two formal explications of Carneades' thesis. The former amounts to saying that one should accept the strongest proposition h which maximizes (a suitably defined notion of) expected truthlikeness. According to the second explication, one should believe h just in case the evidence makes sufficiently probable, not that h is true (as for the Lockean thesis), but that h is truthlike (to a suitably chosen degree).

As I argue, both readings of Carneades' thesis illuminate both the Preface and the Lottery paradoxes, which are given a unified solution within my account (cf. also Cevolani 2016, Cevolani and Schurz 2016). This suggests how to recover an account of full belief within a probabilistic framework while eschewing the problems raised by the Lockean thesis. A consequence of my proposal is that it can be rational to believe propositions with low probability. This is in agreement with some other solutions to the Lottery paradox (e.g., Lin and Kelly 2012), but seems to fly in the face of our intuitive notion of belief. I defend my solution against this and related objections, by highlighting some interesting connections between my two explications of Carneades' thesis, on the one hand, and the Lockean thesis and the distinction between belief and acceptance, on the other hand. I then conclude by comparing my account with Leitgeb's stability theory of belief as based on the Humean thesis, which also provides a different way out of the Preface and Lottery paradoxes.

References

- Cevolani, G. (2016): Fallibilism, verisimilitude, and the Preface Paradox. *Erkenntnis*, 82 (1), 169-183.
- Cevolani, G., Schurz, G. (2017): Probability, Approximate Truth, and Truthlikeness: More Ways out of the Preface Paradox. *Australasian Journal of Philosophy* 95 (2): 209-225.
- Foley, R. (1992): *Working Without a Net: A Study of Egocentric Epistemology*. Oxford University Press.
- Kyburg, H. E. (1961): *Probability and the Logic of Rational Belief*. Wesleyan University Press.
Middletown (Connecticut).
- Leitgeb, H. (2015): I—The Humean Thesis on Belief. *Aristotelian Society Supplementary*, Volume 89(1), 143-185.
- Lin, H., Kelly, K. T. (2012): A Geo-Logical Solution to the Lottery Paradox, with Applications to Conditional Logic. *Synthese* 186(2), 531-575.
- Makinson, D. C. (1965): The Paradox of the Preface. *Analysis* 25(6), 205-207.
- Niiniluoto, I. (1987): *Truthlikeness*. Reidel, Dordrecht.
- Oddie, G. (2014): Truthlikeness. In: Edward N. Zalta (ed.): *The Stanford Encyclopedia of Philosophy*.

Defending non-explanatory understanding: the case of possible explanations

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The aim of this contribution is twofold: on one side to discuss and reject some recent critique against non-explanatory understanding as articulated by Khalifa (2017); on the other side to look at scientific understanding provided through possible explanations and based on the previous discussion to highlight the need to include in an account of scientific understanding a specific characteristic recently emphasized by de Regt & Gijsbers (2017).

In his recent book Khalifa articulates a general model of scientific understanding that subsumes it to the register of explanatory epistemology. Non-explanatory sort of understanding as suggested by Lipton (2009) are discussed in a special chapter as a challenge and danger to the model. The aim is to show that these forms of understanding are subsumable under his model. My argumentation will try to show that the author fails to reach his goal at least in some parts and esp. related to the issue of understanding provided through possible explanations.

A first step in Khalifa's strategy is to reconstruct Lipton's assumptions and argument. I will argue that in both cases; the old version (from 2012 paper) and the last one (2017 book) the reframing is biased in favor of Khalifa's critique. The bias is to be seen esp. in restating of what is called the Lipton's Assumption (LA) that proposes to identify understanding with some benefits of explanations. In the older version, an unjustified existential reading is imposed which assumes the existence of a correct explanation behind any non-explanatory understanding. This reading contradicts Lipton's main move - the 'switch' to other sources of benefits than explanation. In the new LA version, this reading is dropped; nevertheless as I will argue it is restated more diffuse through other means involved in his argumentative strategy.

The central move in Khalifa's critique discloses three main objections under which Lipton's suggestions would succumb. the Right Track Objection, the Wrong Benefit Objection and the Explanatory Objection. The focus will be on the first one as it is the most pervasive one (also for Khalifa) and is directly invoked in the case of possible explanations. It claims that the understanding in Lipton's cases are underdeveloped forms, stations on the track towards the full-blown explanatory understanding. They are instances of proto-understanding - as introduced by Khalifa - and involve the grasp of just explanatory roles of propositions but not of the correct explanation.

Though Lipton's argumentation on possible explanations as providing just modal knowledge on correct explanation might be seen as supporting the right track objection, I will argue that it is just a narrow view on such explanations and their benefits. On one side they provide much more than just such kind of information on actual explanation, on the other this information might not be relevant or important for the understanding provided through actual explanation. An important point is that grasping just explanatory roles does not ensure the end-point of the explanatory inquiry towards the correct explanation. Parts can play explanatory roles for multiple explanations belonging to different 'right' tracks; the actual explanation might never be known or be irrelevant for the expansion of scientific knowledge and understanding. As better understanding must reflect also more successful inferences, so more knowledge & effective knowledge expansion - this might not fall always on the right track for the correct explanation.

Improving and upgrading understanding towards the correct explanation - the central step in Khalifa's strategy - proves to be problematic. Khalifa's justification for the fact that the actual explanation offers greater understanding than any possible one relies the implausible claim that the knowledge of the correct explanation should imply knowledge of all the possibilities. The brief justification offered for this claim remains unconvincing as Khalifa drops the uniqueness of the correct explanation. The quantification in terms of what-if questions as (made more explicit in the old version of his argument) does not save the claim either since it needs it as an assumption to work.

I'll discuss the 2nd example offered by Lipton and Khalifa – the rigged boxing match in which the victory is caused by a lucky uppercut though it would have been obtained later due to the match rigging – which best exemplifies the above points. The understanding gained through a possible

explanation might be greater in some cases, in opposition to ‘the upgrading on the right track’. Directing the inquiry towards rigging practices might improve our understanding and expand our knowledge in a greater way than by knowing that the victory was won just by pure chance. Upgrading towards the actual explanation will bring us therefore less understanding, than moving on the track of the possible explanation.

As an upshot of the discussion we can see that in order to properly approach scientific understanding from possible explanations some further aspects should be taken into consideration. I will point to mainly two suggestions. One is related to the need to enlarge the benefits that Lipton’s claimed for the possible explanations. It is not just modal knowledge for the actual explanation that they might offer; instead we can cash on these benefits through other ways (even in Lipton’s terms involving also knowledge of causality, unity etc). As a scientific illustration, I will refer to the case of agent-based models and the benefits resulted from their use.

The second suggestion is connected to the need of taking seriously the characteristic of fruitfulness and implications in case of understanding from possible explanation. The recent suggestion by de Regt&Gisbers (2017) would this way better capture the specific characteristic of understanding grounded in possible explanations.

The Influence of Prior Probabilities on Judgments about Explanatory Power

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Previous research on the role of priors in explanatory power provides contradicting findings. On one hand, there are studies (e.g., Schupbach, 2011; Schupbach & Sprenger, 2011), which suggest that priors do not have an influence on judgments about explanatory power. On the other hand, recent research shows that this is not necessarily the case, and that judgments about explanatory power are systematically affected by the prior probabilities of explanatory hypotheses (Colombo, Bucher, & Sprenger, 2017). Furthermore, the results from these studies remain inconclusive with regard to the question whether people distinguish between the explanatory power of a hypothesis and its degree of confirmation (Glymour, 2015). Lack of an ability to distinguish between explanation and confirmation would make the degree of confirmation a confounding variable in these studies of explanatory power, thus obscuring the actual dependent measure. One possible reason for the lack of such an ability is that participants might have these concepts naturally intermixed. In other words, they might not be aware, either explicitly or implicitly, that there is a distinction to be made between the explanatory power of a hypothesis and its degree of confirmation. While we do believe that priors play a significant role in judgments about explanatory power (as many others; for a survey see Lombrozo, 2012), we also believe that their influence is underlined by the ability of participants to make a distinction between explanation and confirmation in the first place.

Therefore, the aim of our current study is twofold. First, to directly examine whether people are able to distinguish intuitively between explanatory power and degrees of confirmation. Second, to test whether their judgments of explanatory power change as a function of priors. To address these questions, we designed a series of experiments with adults. In the first stage of our research, we test

participants' intuitions by contrasting an experimental group of subjects, explicitly trained to distinguish between explanation and confirmation, against an untrained control group. In the second stage, we present a set of hypotheses to several groups of participants, but this time we vary the amount of prior information about the hypotheses in the set. This way we can closely track whether and how much judgments about explanatory power change as a function of prior information.

As for our first stage of research, we are equally interested in evidence in confirmation of our alternative hypothesis, i.e., that people can intuitively make the distinction between explanation and confirmation, as well as in evidence for our null hypothesis, which is that people cannot naturally make such a distinction. (In the latter case, however, they can be trained to do so.) That is why we use Bayesian model comparison, as it can provide us useful information both for our alternative and null hypotheses. With respect to the second research stage, given that people can successfully make the distinction between explanation and confirmation (either naturally or by training), we expect that the strength of priors will be predictive for the strength of explanatory power (as reported by Colombo et al., 2017). The research is currently ongoing and the results will be analyzed and discussed by the time of the conference.

References

- Colombo, M., Bucher, L., Sprenger, J. (2017): Determinants of judgments of explanatory power: credibility, generality, and statistical relevance. *Frontiers in Psychology*, 8, 1430. doi:10.3389/fpsyg.2017.01430
- Glymour, C. (2015): Probability and the Explanatory Virtues. *The British Journal for the Philosophy of Science*, 66 (3), 591–604. doi:10.1093/bjps/axt051
- Lombrozo, T. (2012): Explanation and abductive inference. *Oxford handbook of thinking and reasoning*, 260–276.
- Schupbach, J. N. (2011): Comparing Probabilistic Measures of Explanatory Power. *Philosophy of Science*, 78 (5), 813–829. doi:10.1086/662278
- Schupbach, J. N., Sprenger, J. (2011): The Logic of Explanatory Power. *Philosophy of Science*, 78 (1), 105–127. doi:10.1086/658111

About the (im)perfection of the best mathematical explanations in science - the cicada case versus the Königsberg bridge case

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Platonists from the field of the philosophy of mathematics do not often use explicit arguments to defend their views. The Enhanced Indispensability Argument (EIA) formulated by Alan Baker (Baker, 2009) is in that sense a rarity. The argument takes the following form:

- (1) We ought rationally to believe in the existence of any entity that plays an indispensable explanatory role in our best scientific theories.
- (2) Mathematical objects play an indispensable explanatory role in science.
- (3) Hence, we ought rationally to believe in the existence of mathematical objects. (Baker (2009), p. 613)

Baker, using the Enhanced Indispensability Argument, emphasizes that mathematical objects play an indispensable explanatory role in science. There are several characteristic examples in

literature that illustrate such a role – for instance, cicadas, honeycombs, Königsberg's bridges, and the asteroid belt around Jupiter. We will highlight two of these examples and show that they are very different in their strength and (im)perfection, although both are recognized by the scientific community as the best scientific explanations of particular scientific/physical phenomena. More precisely, we will show that the cicada case has serious shortcomings compared with the case of Königsberg's bridges, due to which the former is extremely unconvincing as an illustration of the indispensable explanatory role of mathematics in science.

An example of a “perfect” mathematical explanation is the example of the mathematical explanation in the case of the so-called Seven Bridges of Königsberg. To remind the reader of this 18th-century problem, the issue was whether it is possible to find a path to cross each of the seven bridges only once. The answer to this question is negative. The only known solution for this problem is an explanation in which the objects of graph theory are used (Pincock (2012), pp. 51-53). Namely, this practical situation from the physical world can be presented/mapped with a concrete graph in which there are four nodes and seven connections. Thus, the practical question about the concrete physical possibilities of moving in space is reduced to a theoretical question, that is, to the theorem of graph theory describing the properties of all the concrete graphs, including this one in which the Königsberg case was presented.

Why can we say that this is a “perfect” mathematical explanation? The problem is explained and solved only by mathematical means. In other words, no other non-mathematical tools or details are relevant to the explanation of the problem. A function is defined between the space of objects from the physical world and the space of objects from the mathematical world, that is, a bijection. This function guarantees that if a claim in the mathematical space is true then the corresponding assertion in the physical world must also be true.

On the other hand, in the case of the cicadas we can not say the same thing. Let us remind the reader of that famous example, a common case, used to illustrate the mathematical explanation of an empirical phenomenon:

The example featured the life cycle of the periodical cicada, an insect whose two North American subspecies spend 13 years and 17 years, respectively, underground in larval form before emerging briefly as adults. One question raised by biologists is: why are these life cycles prime? It turns out that a couple of explanations have been given that rely on certain number theoretic results to show that prime cycles minimize overlap with other periodical organisms. Avoiding overlap is beneficial whether the other organisms are predators, or whether they are different subspecies ... (Baker (2009), p. 614)

For example, a prey with a 12-year cycle will meet - every time it appears - properly synchronized predators appearing every 1, 2, 3, 4, 6 or 12 years, whereas a mutant with a 13-year period has the advantage of being subject to fewer predators. (Goles et al (2001), p. 33)

There are several reasons why the previous explanation is far from perfect. In this summary we list just three important points:

1) Unlike in the case of the Königsberg bridges, the mathematical explanation is only part of the explanation in the cicada case, and the mathematical apparatus and its use in the explanation make the explanation only probable, but certainly not safe. Indeed, it is theoretically possible, but not unlikely, that the length of the cicadas' lifecycle is a prime number quite accidentally. This option is not excluded from any argument in the cicada case.

2) Why would the assumption about predatory cycles be correct? Why would predators have to have cycles? We do not have evidence from biologists that there are predators with cycles, that is, predators probably live a continuously undefined number of years. For example, mathematical biologist Horst Behncke writes that “the main disadvantage of [this] model is the fact that in nature there is no apparent predator with similar periodic activity.” (Behncke 2000, p. 417) So, if we do not

have predators with cycles, the whole explanation is meaningless, because the cicada would be eaten after 13 years, as well as after 12, that is, the number of years in its cycle will not affect its survival.

3) Suppose, however, that there are predators with cycles. Does the periodicity of cicadas expressed by a prime number mean an advantage in survival in this case? Yes, because the LCM (least common multiple) is the product of the number of the years of cicadas and predators, it is maximized. The LCM is a product of these numbers if the predator's cycle is shorter than the cicada's cycle. However, we do not have evidence for this from biologists either. If the cycle of cicadas is shorter than the predator cycle, then even if the cycle of cicadas is the prime number x , the LCM does not have to be a product (for example, if the predator cycle is $2x$, the LCM is $2x$, and not $2x2$). In this case, overlapping would be maximized if the predator's cycle is a prime number of years, regardless of whether the cicada's period is a prime or a composite number of years.

References

- Baker, A. (2009): Mathematical explanation in science. *British Journal of Philosophy of Science*, 60, 611–633.
- Behncke, H. (2000): Periodical cicadas. *Journal of Mathematical Biology*, 40, 413–431.
- Goles, E., Schulz, O., Markus, M. (2001): Prime number selection of cycles in a predator-prey model. *Complexity*, 6(4), 33-38.
- Pincock, C. (2012): *Mathematics and scientific representation*. Oxford: Oxford University Press.

Doubting the argument from constraining effects against causal closure

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In a recent paper William Bechtel emphasises the role of feedback-based constraints to clarify and extend his earlier attempts to show why higher mechanistic levels are causally autonomous. He aims to answer recent critics (including Fazekas & Kertész, 2011; Soom, 2012; Rosenberg, 2015) who criticized his approach to levels and his view concerning causal autonomy. A common point of these interpretations was that mechanistic explanations are inherently committed to the inheritance of causal powers from lower levels. This paper tries to show that same line of argument can be extended to answer the new constraint based argument Bechtel develops in his 2017 paper.

To advance the debate the paper provides a detailed analysis and explores the prospects of already known arguments for higher-level causal autonomy available for the proponents of the mechanistic framework. Bechtel and others argue for causal autonomy relying on an entangled mixture of different arguments, so the first goal of this endeavour is to disentangle the relevant parts of the literature, and to reconstruct the different arguments in play.

After setting the stage by briefly introducing the mechanistic programme and discussing what arguing for the causal autonomy of higher-levels amounts to the investigation focuses on the premises and commitments that drive mechanists towards the idea of higher-level causal autonomy. It will distinguish three different arguments: (a) a context-based, (b) an organisation-based and (c) a constraint-based type. These points are independently motivated by different commitments.

After an overview of context-based and organisation-based ideas and known counterarguments to them the paper will turn to Bechtel's new argument for autonomy that belongs to the constraint-based type. Bechtel states that feedback loops like those involved in circadian mechanisms are special as a kind of internal constraint formation takes place in the process. Arguments based on similar ideas go way back to Campbell (1974), Polanyi (1968) and others. Like Bechtel, these theorists based their

ideas of downward causation and emergence on the special role they identified for constraints in the workings of complex systems. What makes Bechtel's approach especially interesting in the contemporary context is that he utilizes a similar constraint based interpretation of the circadian oscillation case study to defeat Kim's well-known exclusion argument against causal autonomy. His target is the first premise of the exclusion argument, the causal closure of the physical.

According to Bechtel the behaviour of mechanistic parts of mechanisms like the circadian system can only be determined relying on constraints (constantly changing internal boundary conditions). Dynamical laws combined with the properties of the parts would underdetermine the behaviour of parts even in analytic mechanics; this is even truer in complex systems. The set of possible constraints is not closed and as a result the set of possible ways the behaviour of parts react to these locally determined limitations isn't closed either. For Bechtel this means that the physical realm, contrary to what the closure principle says, is "extremely open-ended" and "the phenomenon described as top-down causation is not unusual, but common".

The first problem with this argument is that Bechtel's conceptualisation of closure is orthogonal to Kim's. The causal closure of the physical implies at least two things with respect to basic dynamical or causal laws (following Hendry 2010): (1) dynamical laws apply to all things, so they are ubiquitous, (2) there is nothing else that applies to at least some things, so physical laws are exclusive. To contradict Kim's closure premise, the view presented by Bechtel should say something like this: one can constrain parts of a system in a myriad of different ways, so basic physical laws are either not ubiquitous or not exclusive. But he says none of these; he only emphasizes how parts can exhibit different behaviours under different constraints. This only proves the failure of closure if there are extra reasons to think that constraints don't originate from physical processes outside of the system under investigation, but Bechtel seems to accept (1) and (2) and provides no extra reasons.

The second issue the paper raises is that Bechtel argues for top-down causation driven by constraints that somehow belong to the whole. The changing role of parts as a result of changing constraints in feedback loops forms the basis of the argument. This paper argues that the role of constraints in such a scheme can be reconceptualised in straightforward causal language. Constraints are the background conditions for the causal processes, relations that comprise the mechanism as a whole. As a causal process in a mechanism goes to completion the inner condition of the system changes. This means new background conditions as well that results in the formation of new causal relations and processes while others vanish. So, the language of constraints can be translated to the language of causes and it can be shown that it is never the whole mechanism that constrains certain parts of itself; it is always other parts of the same system that does the constraining and the whole plays no autonomous role.

Minimal structure explanations, scientific understanding and explanatory depth

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In this talk, I outline a heuristic for thinking about the relation between explanation and understanding that can be used to capture various levels of "intimacy", so to speak, between them, i.e. by using this heuristic we will be able to explain away some of the seemingly paradoxical cases in which it is claimed we could have the understanding without explanation, as well as cases where there can't be understanding without explanation. The idea is that the level of complexity in the structure of explanation is inversely proportional to the level of intimacy between explanation and understanding, i.e. the more complexity the less intimacy, and vice versa. The structure of explanation should be

understood as a description of the exact relation between the explanans and explanandum, and the complexity in this context should be understood as the number of components that are required to describe this relation. In this sense, the complexity could possibly be measured, probably by using something like the minimum description length principle (Baron and Cover 1991; Baron et al 1998; Grünwald 2007), but developing such measure is out of the scope of this paper, because the primary goal of this paper is to point out the dependencies between the structure of explanation, scientific understanding and explanatory depth. I further argue that the level of complexity in the structure of explanation also affects the explanatory depth in a similar way to intimacy between explanation and understanding, i.e. the less complexity the greater explanatory depth and vice versa.

However, it is very important to distinguish what is minimal and what is complex in this context. The structure of explanation that can be very complex or minimally complex, not the explanation itself. Also, there is an important difference between simple and minimal here, in the sense that an explanation can be simple, but have a very complex structure, e.g. any explanation that has a deductive-nomological (D-N) structure. On the other hand, an explanation can be very complicated, but have a minimal structure, e.g. a topological explanation.

On this view, there are degrees of complexity in the structure of explanation, and so there could be very complex explanations which require a great deal of mediating knowledge to grasp the exact relation between the explanans and explanandum.

To avoid circularity when using the terms “grasping” and “understanding” in referring to the structure of explanation, following Strevens (2008, 2013) and Khalifa (2017) I distinguish between “understanding-that” and “understanding-why”. Understanding-that refers to some basic cognitive abilities such as being a competent speaker of a language, knowing what certain mathematical relations mean, grasping the mathematical axioms and knowing what it means to say that they are logically primitive, or knowing that something is a fact. For example in the DN model there is also the understanding-that of the rules of inference, order of derivation, validity and soundness. The understanding-why refers to knowledge of why something is the case, which is based on the knowledge of counterfactuals.

But the understanding-why comes from knowledge of all these relations and it is also supported by counterfactual thinking. Another way to put it is that the understanding-why comes from the structure of explanation, and it has a form of counterfactual information about the dependency relations between the explanans and explanandum.

What makes some structure of explanation more complex, is not the amount of background assumptions, but the number of components that are required to describe the relation between explanans and explanandum. In this sense, it means the more components the more complex the structure of explanation, and vice versa. For example, in the D-N model of explanation (Hempel and Oppenheim 1948) besides the statements about antecedent conditions and general laws, there are several other components that play an important role in the derivation of the explanandum, these are: the rules of inference (modus ponens, modus tollens), order of derivation (what is derived from what), soundness and validity of the argument. This kind of description of explanatory relations allows that there could be false explanation that provides correct understanding of explanatory relations. For example, if we substitute the Phlogiston theory as a general law in the D-N model, we will still be able to understand various counterfactual dependencies that the model postulates, and thus to have a correct understanding-why despite having a false explanation.

The complex structure of explanation can be represented schematically in the following way:

(CSE): $\text{Understanding}_{\text{that}}(X,Y,Z,W) \rightarrow \text{Understanding}_{\text{why}}$

Where X,Y,Z and W in the D-N model may represent antecedent conditions, general laws, validity, soundness, order of derivation, and some additional explanatory component respectively.

Based on all these explanatory components we are able to derive the explanandum from the explanans and to grasp various counterfactual dependency relations, i.e. to understand-why.

Whereas in minimal structure explanation just by understanding-that of the mathematical dependencies that describe a topology (in the case of topological explanation), we are able to understand various counterfactual dependencies in the very same noetic act of grasping the description of topology, and thus to have almost unmediated understanding-why.

The schematic representation of the minimal structure explanations would then look like this:

Minimal structure explanation

(MSE): *Understanding_{that}(T) → Understanding_{why}*

Where T is a description of mathematical dependencies in a certain topology.

The minimal structure explanations also support an account of explanatory depth, that can be applied to both causal and non-causal explanations. The explanatory depth should be understood in terms of richness of counterfactual explanatory relations that the explanation provides, so in this sense, the explanations which provide fewer counterfactual explanatory relations are less deep than the ones that provide more counterfactual relations.

Depending on the complexity of the structure of explanation, the relation between explanation and understanding can be more intimate or less intimate, the more complex the structure of explanation the less intimate the relation between the explanation and understanding, and vice versa. Because of the minimal structure and more direct relation between explanation and understanding, these explanations will be deeper, and more universal, because they will provide more counterfactual dependency relations for our grasping.

Acknowledgments

This research is funded by the European Commission, Horizon 2020 Framework Programme, Excellent Science, Marie Skłodowska-Curie Actions under REA grant agreement no 703662; project: Philosophical Foundations of Topological Explanations (Proposal acronym: TOPEX).

Instrumental Realism

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The term instrumental realism was introduced in 1991 by Don Ihde as a position characterizing a group of philosophers of science – Robert Ackermann, Hubert Dreyfus, Ian Hacking, Don Ihde and Patrick Heelan – who shared a realistic position in philosophy of science and an interest in the instrumental aspect of science. The thesis of instrumental realism is that sciences have access to independent reality, but this access is indirect, mediated by instruments. The aim of the presentation will be to develop this view further and especially to outline its semantic structure.

According to instrumental realism reality represented by science has the structure similar to a differentiable manifold. It is formed by clusters of theories and models. These clusters resemble coordinate charts of the manifold. The notion of the manifold has three important aspects. First of all, it shows that in order to obtain a representation of reality, one does not need any unified picture, any all comprehensive model, any universal framework. The representation can be fragmentary and it still may be sufficient for comprehensive understanding of reality. Secondly, it is not necessary, that all the

fragments are compatible, or that there exists a neutral language into which all fragments can be translated. It is sufficient, that between any two neighboring fragments there are mappings that enable translation in both directions between the overlapping parts. And thirdly, there need not to be any universal semantics, determining for each fragment in a unique way the meaning of its linguistic expressions. It is sufficient, that every fragment is rooted in a particular instrumental practice.

Every instrument has limits of resolution and so it opens access to reality on a particular scale of magnitudes. So a phenomenon or a feature of reality typically appears only when the instrumental practice becomes sufficiently sensitive so that it can detect this phenomenon; and often it is lost when the sensitivity of instruments increases so that finer structures become detectable and so the original phenomenon becomes a rough feature of this finer structure. Instrumental realism develops its semantics along the line "meaning is use". Thus it defines the meaning of any notion occurring in a particular theory (that belongs to a particular cluster of theories) on the basis of the use of this notion in the instrumental practice, in which that particular cluster is rooted. This is important, because often (actually it is the rule) after the transition to a new instrumental practice many notions change their meaning in a radical way.

That is why the semantics of instrumental realism is fragmentary. According to instrumental realism there is no all-comprehensive meaning of any scientific notion. Scientific realism reacts to this threat of relativism by accepting only the notions (and ontological commitments) of our best scientific theories. Instrumental realism is in this respect more liberal, and it accepts the semantics and the ontological commitments of overthrown theories (such as Newtonian mechanics, caloric theory of heat or geometric optics). Nevertheless, it relates the semantics of these theories to their own instrumental practice. Thus it believes, that despite the fact, that Newtonian mechanics was refuted and overcome by relativistic mechanics, this happened by means of instruments (like subtle interferometers), that were unavailable in the time, when Newtonian mechanics was constructed, and do not belong to its instrumental practice. Therefore in the realm of phenomena revealed by means of the instrumental practice of Newtonian mechanics (characterized by pendulums, telescopes, and similar instruments) we can still use the notions of Newtonian mechanics in their representational role. If we base meaning not on some mysterious relation between words and reality, but on the practical use of instruments, we can build a fragment of semantics of Newtonian physics. The cases of caloric theory of heat or of geometrical optics are analogous. In every case we must first determine the instrumental practice, which we are using, and then we can choose the adequate fragment of semantics.

Despite of this loosening the criteria for meaning, instrumental realism does not lead to any relativism, because it requires that in the realms, where any two instrumental practices overlap, there be some mappings (usually having the form of some limit transitions), which make it possible to translate the representation of the phenomena from one coordinate chart to the other. These mappings have usually the form of refinements (one practice being much more sensitive than the other).

References

- Ackermann, R. (1985): *Data, Instruments, and Theory*. Princeton University Press, Princeton.
- Cartwright, N. (1983): *How the Laws of Physics Lie*. New York: Oxford University Press.
- Chang, H. (2004): *Inventing Temperature: Measurement and Scientific Progress*. New York: Oxford University Press.
- Dreyfus, H. L. (1972): *What Computers Can't Do*. Harper and Row, New York.
- Hacking, I. (1983): *Representing and Intervening*. Cambridge University Press, Cambridge.
- Heelan, P. (1983): *Space Perception and the Philosophy of Science*. University of California Press, Berkeley.
- Ihde, D. (1991): *Instrumental Realism: The Interference between Philosophy of Science and Philosophy of Technology*. Indiana University Press, Bloomington.

- Morgan, M. S. and Morrison, M. (1999, eds.): *Models as Mediators: Perspectives on Natural and Social Science*. Cambridge: Cambridge University Press.
- Psillos, S. (1999): *Scientific Realism: How Science Tracks Truth*. Routledge, London.
- Weisberg, M. (2013): *Simulation and Similarity: Using Models to Understand the World*. New York: Oxford University Press.

Poincaré's one conventionalism

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Recent Poincaré scholarship distinguishes between several uses of the term „convention“ in his works. de Paz (2014) points out that the axioms of geometry and the principles of mechanics are not the only categories called „conventions“. „Disguised definitions“ and „implicit axioms“ and „indifferent hypotheses“ and others are also „conventions“. Stump (2015) and Ivanova (2015) argue that there are two radically different kinds of conventions, the geometrical and the physical, and that these kinds are to be distinguished in order to properly understand Poincaré's role in the development of 20th century philosophy of science. These readings are influential, so much so that other scholars can casually claim, e.g., that “Poincaré used the term ‘convention’ in various overlapping ways” (Schmaus 2017).

It is the purpose of this paper to dispute this interpretation. I will argue that Poincaré uses the term “convention” in precisely one sense. However, this sense is functional. For everything Poincaré calls conventional, he applies a clearly definable “conventionalisation” strategy. This strategy comprises of two tactical movements. One of them is constructive. We begin from sense data and see how far we can get with what is given in experience. At every point at which we must go beyond this given, we adopt a convention. We do not do so not because experience forces us to do so, or because it is logically required. We adopt the convention for two reasons. First, it fits what is given in experience without encumbering us with its entire multiplicity (thus being convenient). Second, it enables us to make more and more complex reasoning about what is given in experience (thus being useful).

A second movement is dialectical. It consists in basic, but systematic applications of reductiones ad absurdum. For any given judgement about to be “conventionalised”, we examine three hypotheses: that it is analytic a priori; that it is synthetic a priori; that it is synthetic a posteriori. We then show how each of these hypotheses leads to unacceptable conclusions. But if an axiom is neither analytic, nor synthetic a priori, nor synthetic a posteriori, then it must have a different epistemic status. Poincaré calls this status “convention”.

I will illustrate Poincaré's strategy by an analysis of his arguments on the axioms of geometry – in “Sur les hypothèses fondamentales de la géométrie” (1887) and “On the foundations of geometry” (1898) – and on the principle of inertia – in *La science et l'hypothèse* (1902). Then, I will trace the origin of the currently dominant understanding of Poincaré's conventions to an overemphasis on what Poincaré identifies as the sources of conventions. Clearly, „convention“ denotes an epistemic status. Describing a proposition as conventional is, for Poincaré, making a claim about our knowledge of it. Thus, we must interpret conventions as belonging essentially to the context of justification. This ought to preclude appeals to differences in the genetic contexts of geometric axioms and physical principles in the interpretation of the concept „convention“. Among these appeals are those to the role of empirical data, the hierarchy of sciences Poincaré proposes and also to chronological relations in Poincaré's rational reconstructions.

This functional understanding of Poincaré's concept “convention” has two important advantages. First, it unifies what is currently seen as a multiplicity of uses of the term. Thus, it shifts the

locus of Poincaré research away from questions like “how many kinds of convention are there?” which have long dominated the field to the detriment of other viable research avenues. Second, it offers an understanding of Poincaré’s philosophy which is more in tune with his general grouptheoretic epistemic approach. Conventions are not to be understood in terms of their material properties, which may well vary for reasons pertaining to their specific place in the structure of a scientific domain, or to the historical context of their adoption. Instead, we will be able to see conventions as judgements of a kind fitting certain argumentative operations.

References

- de Paz, M. (2014): The Third Way Epistemology: A Recharacterization of Poincaré’s Conventionalism. In: *Poincaré, Philosopher of Science. Problems and Perspectives. The Western Ontario Series in Philosophy of Science*, 79:47–66.
- Ivanova, M. (2015): Conventionalism, structuralism and neoKantianism in Poincaré’s philosophy of science. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 52:114–122.
- Poincaré, H. (1887): Sur les hypothèses fondamentales de la géométrie. *Bulletin de la Société mathématique de France*, 15:203–216.
- Poincaré, H. (1898): On the foundations of geometry. *Monist*, 9:1–43.
- Poincaré, H. (1902): *La science et l’hypothèse*. Flammarion, Paris.
- Schmaus, W. (2017): Henri Poincaré and Charles Renouvier on Conventions; or, how Science is like Politics. *HOPOS: The Journal of the International Society for the History of Philosophy of Science*, 7:182–198.
- Stump, D. (2015): *Conceptual Change and the Philosophy of Science: Alternative Interpretations of the a Priori*. Routledge, New York.

‘Local realism’ and the misconceptions about scientific realism

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The discussion about realism in the area of philosophy of quantum mechanics (QM) has in most cases been closely related to the subject of the interpretation of the theory, and it is common knowledge that two very important theorems have played a key role: Bell’s and Kochen-Specker’s.

The Kochen-Specker and Bell theorems constitute undeniable proofs that a reinterpretation of QM, avoiding serious contradictions with the classical worldview –that is, the classical physics view of the world–, is not possible. Therefore, as a result of these two theorems, the main discrepancies with the classical worldview are,

D1. Classical physics describes a world of separated systems with separated states whereas quantum physics describes the world at the quantum level as non-separable.

D2. Some properties of quantum entities exhibit causal dependence on the context of measurement or observation. In other words, at the quantum level we have violation of the “possessed values principle” or “principle of faithful measurement” (the two principles are almost identical and state that the values of observables exist prior to their measurement and that measurement simply reveals them) and of the “precise values principle” (all observables possess definite sharp values at all times).

The formulation of Bell’s theorem and its different versions inspired a series of experiments in order to test, eventually, a whole class of theories: the so-called ‘local realistic theories’. The

combination of Bell's theorem with the experimental results, which confirmed the predictions of QM, established, in favour of QM, the incompatibility of the theory with 'local realism'. I will trace the formulation of this type of 'realism', as presented and developed in the literature of philosophy of physics. I will mainly mention the early cases –Clauser and Horne (1974), Clauser and Shimony (1978), Aspect et al. (1981) and (1982), Jarrett (1984) and (1989), Redhead (1987) and (1995), and also Leggett (2003) and (2008)– because they set the stage and stimulated quite a number of followers.

In all the above cases, 'local realism' epitomizes an anticipated standard physical behavior comprised of features associated with classical physics. So, representative examples from the literature illustrate the fact that the authors: (1) refer to 'realism' as the view that external reality has definite properties at all times and independently of any measurement; and (2) associate 'local realism' with classical physics.

Even if the aforementioned views were developed in the context of 'local realism', they reflect the beliefs and intuitions about realism of those who formulated them –use of the same term cannot be coincidental; hence, they shaped within the community of physicists and philosophers of physics the perception of the relation between quantum physics and realism. This perception of the said relation seemed to suggest that quantum physics refutes realism. But this is ill-founded because it is based on 'local realism' –which is closely related to the classical worldview– and not on the scientific realism advocated by philosophers of science. The dominant understanding of scientific realism does not involve separability or non-separability issues, and does not necessarily presuppose the possessed values principle as 'local realism' does. Scientific realism consists in acknowledging what experiments and theorems indicate and in recognizing the fact that –in the case of micro-entities–certain state properties cannot be considered as well-defined independently of their measurement context; to wit, realism consists in the acceptance of what our best scientific theories tells us, approximately of course, about the structure or the nature of the world. Hence, we must make the distinction of the two and not consider the failure of 'local realism' as outright failure of realism tout court.

References

- Aspect, A., Grangier, P., Roger, G. (1981): Experimental Tests of Realistic Local Theories via Bell's Theorem. *Physical Review Letters* 47: 460-463.
- Aspect, A., Dalibard, J., Roger G. (1982): Experimental Tests of Bell's Inequalities Using Time-Varying Analyzers. *Physical Review Letters* 49: 1804-1807.
- Clauser, J. F., M. A. Horne (1974): Experimental consequences of objective local theories. *Physical Review* D10: 526-535.
- Clauser, J. F., Shimony, A. (1978): Bell's theorem: experimental tests and Implications. *Reports on Progress in Physics* Vol. 41: 1881-1927.
- Jarrett, J. P. (1984): On the Physical Significance of the Locality Conditions in the Bell Arguments. *Noûs* 18: 569-589.
- Jarrett, J. P. (1989): Bell's Theorem: A Guide to the Implications. In: Cushing, J. – McMullin, E. (eds.): *Philosophical Consequences of Quantum Theory: Reflections on Bell's Theorem*: Notre Dame, Indiana: University of Notre Dame Press. 60-79.
- Leggett, A. J. (2003): Nonlocal Hidden-Variable Theories and Quantum Mechanics: An Incompatibility Theorem. *Foundations of Physics* 33: 1469-1493.
- Leggett, A. J. (2008): Realism and the physical world. *Reports on Progress in Physics* 71: 1-6.
- Redhead, M. (1987): *Incompleteness, Nonlocality, and Realism*. Oxford: Clarendon Press.
- Redhead, M. (1995): *From Physics to Metaphysics*. Cambridge: Cambridge University Press.

In defence of veridicality pragmatic truth and scientific understanding

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The question I am addressing is in virtue of what does a scientific explanation provide understanding of its explanandum? My answer is somewhat unsurprising – understanding has a veridical and an inferential component. The veridical component is that the explanans should be a sound argument which means that its premises should be true. The inferential component is that since scientific explanations involve grasping new concepts and inferential patterns, I have derived understanding of the phenomena based on the explaining theory if I have managed to master relevant features of the theory which correspond to relevant features of the explanandum in such a way as that by operating with the inferential apparatus of the theory, the conclusions I obtain about the explanandum will partially fit the facts.

Due to this partiality the resulting inferences cannot be assessed using classic correspondence criteria of truthfulness. In order to meet this challenge I adopt da Costa's formal rendering of pragmatic or partial truth. Non formally pragmatic truth can be seen as a theory of scientific acceptance where a proposition is accepted as pragmatically true if:

- 1.) it directly corresponds to observation;
- 2.) or some of the basic statements it implies are true in the sense of 1.

The pragmatic truth's main virtue is that it manages to capture the natural intuition that scientific theories should be factually informative, whilst also acknowledging the provisional and incomplete character of ongoing theoretic development. Therefore by defining the veridicality condition of understanding in terms of partial truth we can assess explanatory arguments as providing understanding of the explanandum based not only on their validity (being correct inferences from a theory) but on their soundness (being valid and at least partially factually accurate).

Naturally I have to address criticisms against the veridicality condition which come from what can be broadly conceived as non-factual accounts of understanding. The non-factual accounts core claim is that the basic building blocks of understanding lies in obtaining certain abilities, namely that I possess understanding of some phenomenon if my beliefs about this phenomenon enable me to generate practical conclusions about this phenomenon. Here truthfulness plays a secondary role since understanding is analysed as an instrumental affair – an ability to predict and control the phenomena, to infer some technically applicable conclusions about it, as a heuristic virtue that leads to a development of new and better theory or as the ability to navigate within a possibility space associated with the theory.

The merit of these accounts, as their proponents claim, is that they agree with the historiography of science. Namely with the well recognized fact that past theories which are presently rejected and thus are strictly speaking false, can still bring a limited understanding of their domain. By extension the same claim acknowledges the everyday intuition that children possess some understanding of nature derived from their primary schoolbooks even though virtually all the claims in their textbooks are in important ways incorrect. These types of arguments however present a valid critique only against veridicality as an absolute or complete correspondence. They can be addressed by truthfulness defined as a notion of partial correspondence, therefore retaining a natural criterion which can be used to categorize the difference between explanatory inferences which bring understanding and those that do not.

The difficulty the non-factual accounts face is that by jettisoning the notion of truthfulness they effectively decouple the discourse on understanding from the rich background literature on the logic of explanation. The analyses of explanations broadly investigate explanations as a types of structured arguments where information from the explanans is carried over to the explanandum by some inferential relation. The natural criterion to judge if such arguments are successful in their informativeness is to categorize them as valid or sound, and therefore minimally to assess if they can bring understanding of the explanandum based on their truthfulness.

Criteria as heuristic value and practical applicability however are global features of theories and cannot be directly applied to assess particular explanatory arguments. Predictive success and the ability to navigate within the modal space of the theory can be conceived as derivations based on explanatory arguments but whenever predictions are correct and the modal inferences are informative about the domain of facts they reduce to the pragmatic criterion of factual accurateness and therefore can be accounted for by partial truthfulness.

I will further support my argument by analysing the evolution of predator-prey models and the debate on the paradox of enrichment – a valid conclusion about predator-prey dynamics derived from a modified version of the Lotka-Volterra equations. According to the paradox in some situations enrichment of food resources available to the prey will lead to the destabilization of the predator population and therefore to the possible extinction of both populations or at least of the predator population.

However the paradox of enrichment remained a purely theoretic artifact with virtually no direct support from observations based on natural populations. Due to the established status of the model the paradox has been treated as a factual outcome thus instantiating what seemed as a legitimate explanatory question: “Why do natural populations of predator and prey display stability in enrichment scenarios?”

Several additions to the model were proposed, as factors blocking the predicted outcome, these however remained ad hoc extensions of the model based on phenomenological observations. Eventually the paradox has been assessed as a valid inference based on a representationally inaccurate model. The model however has not been “falsified” as it still provides some factually accurate representations of population dynamics.

Based on this case I conclude that the general theoretic models and the concepts and relations they instantiate provide understanding of the phenomena whenever the inferences based on them fit the facts and they lack explanatory power and cannot provide understanding whenever they generate anomalous conclusions that have no observational support. Therefore the conclusions drawn from these models can be accepted as partially true.

Abstraction in Scientific Modeling

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Abstraction is ubiquitous in scientific model construction. It is generally understood to be synonymous to omission of features of target systems, which means that something is left out from a description and something else is retained. Such an operation could be interpreted so as to involve the act of subtracting something and keeping what is left, but it could also be interpreted so as to involve the act of extracting something and discarding the remainder. The first interpretation entails that modelers act as if they possess a list containing all the features of a particular physical system and begin to subtract in the sense of scratching off items from the list. Let us call this the omission-as-

subtraction view. According to the second interpretation, a particular set of features of a physical system is chosen and conceptually removed from the totality of features the actual physical system may have. Let us call the latter the omission-as-extraction view.

If abstraction consists in the cognitive act of omission-as-subtraction this would entail that scientists know what has been subtracted from the model description and thus would know what should be added back into the model in order to turn it into a more realistic description of its target. This idea, most of the time, conflicts with actual scientific modeling, where a significant amount of labor and inventiveness is put into discovering what should be added back into a model. In other words, the practice of science provides evidence that scientists, more often than not, operate without any such knowledge. One, thus, is justified in questioning whether scientists actually know what they are subtracting in the first case. Since it is hard to visualize how modelers can abstract, in the sense of omission-as-subtraction, without knowing what they are subtracting, one is justified in questioning whether a process of omission-as-subtraction is at work.

One modeling practice requires the construction, use or appropriate modification of theory-driven models. For example, the model for the particle in a spherically symmetric potential in quantum mechanics. When in this model it is assumed that a particle is under the influence of a spherically symmetric potential we know that this involves several omissions, but we don't know what these are and we cannot find out until it is decided to which physical system this model will be applied. Once the target system is decided, a lot of research goes into determining exactly those omissions that are implicit in the model and which must be brought back into the model in order to turn the latter into a representation of the target system. To put it differently, the omissions are not given from the outset; they must be discovered. The omissions involved in such a theory-driven model are not specific to a particular target system, because such a model is not meant as a representation of a particular system. Theory-driven models are models of theoretical types that could, with the proper refinements or adjustments, be applied to different physical systems or be turned into representations of different systems. For each different application it would require different addenda to the initial theory-driven model in order to construct an appropriate representational model. A sense of abstraction as omission-as-subtraction cannot help us grasp the practice of constructing, refining and applying theory-driven models.

Another kind of model-building practice in Quantum Mechanics requires the construction of phenomenological models. These are models that are not direct descendants of theory, but are constructed by a synthesis of theoretical principles, other auxiliary hypotheses and semi-empirical results. One example is the liquid-drop model of the nuclear structure. The general hypothesis that underlies the model is that nuclei are strongly bound collections of nucleons that only perform collective motion in analogous manner to that of a liquid-drop. This hypothesis involves several omissions some of which were not known to, nor anticipated by, working scientists. The research program of models of the nuclear structure, amongst other things, involved research for discovering what corrections should be made to the Hamiltonian of the liquid-drop model, i.e. how to adjust for the initial omissions implicit in its underlying hypothesis, in order to make it a more realistic representation of the nuclear structure and thus be able to use it to explain nuclear properties. Several were discovered. As an example I mention what nuclear physicists dubbed "giant resonance" that refers to high-frequency collective excitations of nucleons, which had not been thought of before and that was discovered in the process of trying to adjust for the implicit omissions of the model. The point is that giant resonance was among those things left out when scientists pursued the initial assumption that the nucleus behaved as a liquid-drop. The conclusion is the same as for theory-driven models: a sense of omission-as-subtraction cannot help explicate the practice of constructing and refining phenomenological models.

Different modeling practices show that what is involved in the model-building process is the act of extracting certain features of physical systems, conceptually isolating and focusing on them. This is

the sense of omission-as-extraction, that I argue is more suitable for understanding how scientific model-building takes place before the scientist moves on to the question of how to make the required adjustments to the model in order to meet the representational goals of the task at hand.

Kuhn's Incommensurability Thesis: Good Examples Still to Be Found

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In his *The Structure of Scientific Revolutions*, Thomas Kuhn argued famously that scientific revolutions occur in science as well as that they consist in paradigm shifts in which superseded and new paradigms are incommensurable. This amounts to the claim that scientific revolutions cannot be explained within a rationalist model of scientific progress, according to which it is always possible to explain, in one way or another, why new paradigms are better than the superseded ones.

Although Kuhn changed his accounts of incommensurability during time (see, for example, Kuhn 1983; Sankey 1993; Chen 1997 for more details), probably the most controversial (and the most popular) Kuhn's thesis concerning incommensurability was that scientists who work within different paradigms live in different worlds. Relating to this, Kuhn (1970: 150) says: "In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds ... Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction." In his later works, Kuhn replaced the perceptual account of incommensurability with a linguistic account, but the main idea concerning scientific practices in different paradigms remained the same.

In his book, Kuhn provided several examples in order to support his main theses about the nature of scientific revolutions. His favourite (and the most discussed) example was Einstein's special theory of relativity that succeeded Newtonian mechanics since such a revolution led to radical changes concerning our understanding of some of the most fundamental concepts in physics, such as space, time, mass and energy. Generally, Kuhn thought that pointing out good examples in the history of science is a virtue that any acceptable account of the nature of science and scientific change should have.

My aim in this paper is to show that Kuhn did not provide good examples for his main thesis about incommensurability of competing paradigms. In order to do that, I draw the distinction between absolute (or global) and relative (or local) incommensurability, according to which the latter is, in contrast to the former, relative to already fixed criteria (e.g., empirical evidence, meaning of words, non-empirical virtues, and the like). Such a difference can be illustrated by the cases of underdetermination of scientific theories (see Duhem 1906; Carrier 2011 for more details), in which competing (and conceptually different) theories (e.g. Copernicus heliocentrism and Tychonic geoheliocentrism before Galileo's discovery of the phases of Venus and the moons of Jupiter) are capable of explaining the same empirical data. This amounts to the claim that those theories are incommensurable with respect to empirical observation. However, those theories are not absolutely incommensurable, because they could be (as they did) compared with respect to certain non-empirical virtues, such as simplicity, coherence with the background knowledge, testability, fruitfulness and the like. Now, scientists might disagree with respect to those non-empirical virtues as well (see, for example, Newton-Smith 1981: §9.8. for more details). For instance, conventionalists typically prefer simplicity over coherence with the background knowledge, while inductivists do the opposite. But such a disagreement is resolvable (at least in principle) by further considerations (e.g., by establishing research priorities, further philosophical considerations, and the like).

Now, let us turn to Kuhn's favorite example: the special theory of relativity. As is well known, Einstein was aware of the fact that he could choose whether to adopt a more complicated geometry (non-Euclidian spacetime geometry) or to adopt a more complicated physical theory, and that he, as a physicist, preferred simpler physics over simpler (Euclidian) geometry (see, for example, Howard 2005: 38). In view of the last fact, simplicity can be understood as a rational criterion which led physicists to abandon the Newtonian paradigm.

If so, then the cases of local incommensurability do not imply that paradigms are absolutely incommensurable as it was indicated by Kuhn's passage quoted above. It seems that the real support to Kuhn's thesis that "the proponents of competing paradigms practice their trades in different worlds" would require absolute incommensurabilities, but, alas, Kuhn did not provide any evidence that those incommensurabilities ever occurred in the history of science.

References

- Carrier, M. (2011): Underdetermination as an Epistemological Test-Tube: Expounding Hidden Values of the Scientific Community. *Synthese* 180: 189–204.
- Chen, X. (1997): Thomas Kuhn's Latest Notion of Incommensurability. *Journal for General Philosophy of Science* 28: 257–273.
- Duhem, P. (1906): *The Aim and the Structure of Physical Theory*. New York: Atheneum.
- Howard, D. (2005): Einstein as a Philosopher of Science. *Physics Today* 58: 34–40.
- Kuhn, T. (1970): *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press. 2nd ed.
- Kuhn, T. (1983): Commensurability, Comparability, and Communicability. In: Asquith, P., Nickles, T. (Eds.): *PSA 1982, Vol. II, Philosophy of Science Association*. East Lansing, pp. 669–688.
- Newton-Smith, W. (1981): *The Rationality of Science*. Routledge & Kegan Paul: London.
- Sankey, Howard. (1993): Kuhn's Changing Concept of Incommensurability. *British Journal for Philosophy of Science* 44: 759–744.

What, if anything, does counterfactual history teach us about the contingency of science?

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Among professional historians, a long-standing tradition has it that counterfactual speculation provides at best an amusing distraction from the duties of serious scholarship, and at worst an occasion for indulging in all manners of wishful thinking (see, e.g., Carr 1961, Evans 2014). In light of said tradition, one may be tempted to conclude that counterfactual history of science is nothing but a flight of fancy, and that it cannot teach us anything about the nature of the scientific enterprise. In this paper, we argue that such temptation should be resisted and clarify the contribution that counterfactual history can make to the inevitability versus contingency of science controversy.

In recent years, varied philosophical agendas have prompted the deployment of counterfactual histories of biology (e.g., Bowler 2013, Jamieson and Radick 2013, 2017), physics (e.g., Pessoa 2010, 2011), and chemistry (e.g., Chang 2012), among other disciplines. Such counterfactual histories are often believed to support (some version of) the contingency thesis—the claim that history of science might have followed a path leading to alternative, non-equivalent theories, as successful as the ones that we currently embrace—and to rebut the inevitability thesis—the claim that history of successful science went just as it was supposed to go.

Nevertheless, one noteworthy feature of extant counterfactual histories of science is that the outcomes of the imagined alternative developments of the relevant disciplines lie in the close vicinity of the endpoints of the corresponding actual scientific developments, and in some cases plainly coincide with them. This, we suggest, should come as no surprise, since good counterfactual speculations need to be plausible—where “being plausible” means that both the antecedent and the consequent of the counterfactual must exhibit an appropriate continuity with what we know about the world. In the case of counterfactual speculations concerning history of science, the results of actual science—broadly, “scientific achievements that are taken to be reliable” (Soler 2015, p. 46)—are what we know about how the world works, and provide the yardstick for the assessment of the plausibility of the counterfactual. Therefore, we argue, one cannot imagine outcomes far removed from actual history of science without at the same time jettisoning plausibility: one can use counterfactual histories to question the inevitability thesis, but not to support strong versions of the contingency thesis. More specifically, contrary to what some advocates of contingency suggest, counterfactual speculations, as long as they are plausible, can do nothing to support the claim that, as a consequence of a replay of the tape of history of science, tables would be turned drastically, so that today we would embrace a set of theories very different from our current one.

Good counterfactual narratives in history of science are attuned with weak versions of the contingency thesis. Saying that a version of a thesis is weak, however, does not mean to say that it is inconsequential. For instance, Peter Bowler’s speculations concerning how biology might have developed, had Darwin died before putting forward his theory of evolution by natural selection, ultimately end up with evolution by natural selection emerging anyway, although at a different point in time. Indeed, what Bowler aims at exploring is not a world without the theory of evolution, but rather, a world in which, due to its delayed appearance, it is simply impossible for its critics to accuse it of originating social Darwinism. In short, then, the examples of counterfactual history that we discuss suggest that plausible counterfactual narratives have the potential to elicit a change in how we view the relevant pieces of knowledge.

References

- Bowler, P. J. (2013): *Darwin deleted: Imagining a world without Darwin*. Chicago: The University of Chicago Press.
- Carr, E. H. (1961): *What is history?*. London: Macmillan.
- Chang, H. (2012): *Is water H₂O? Evidence, realism and pluralism*. Berlin: Springer.
- Evans, R. (2014): *Altered pasts. Counterfactuals in history*. Waltham: Brandeis University Press.
- Jamieson, A., Radick, G. (2013): Putting Mendel in his place: How curriculum reform in genetics and counterfactual history of science can work together. In: Kampourakis, K. (ed.): *The philosophy of biology: A Companion for educators*. Dordrecht: Springer. pp. 577–595.
- Jamieson, A., Radick, G. (2017): Genetic determinism in the genetics curriculum. An exploratory study of the effects of Mendelian and Weldonian emphases. Forthcoming in *Science & Education*. DOI: 10.1007/s11191-017-9900-8.
- Pessoa Jr., O. (2010): Modeling the causal structure of the history of science. In: Magnani, L. et al. (eds.): *Model-based reasoning in science and technology*. Heidelberg: Springer. pp. 643–654.
- Pessoa Jr., O. (2011): The causal strength of scientific advances. In: Krause, D., Videira, A. (eds.): *Brazilian studies in the philosophy of science*. Berlin: Springer. pp. 223–231.
- Soler, L. (2015): Why contingentists should not care about the inevitabilist demand to “Put up or shut up”. A dialogic reconstruction of the argumentative network. In: Soler, L., Trizio, E., Pickering, A. (eds.): *Science as it could have been. Discussing the contingency/inevitability problem*. Pittsburgh: University of Pittsburgh Press. pp. 45–113.

A Problem for Jeffrey Conditionalizers

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How should a rational agent update her degree of belief (subjective probability) in some hypothesis (H) after learning that E is true? It is almost universally agreed that she needs to update by Bayesian conditionalization. That is, her new subjective probability of H needs to update to her conditional probability of H given E : $\text{Pr}^*(H) = \text{Pr}(H|E)$, where $\text{Pr}^*(\cdot)$ represents the posterior probability function. There are, however, many cases where an agent learns something but becomes certain of nothing that can be expressed (e.g., the classic case of observations by candlelight where the agent becomes 0.70 certain that a piece of cloth is green, 0.25 certain that it is blue and 0.05 certain that it is violet; Jeffrey, 1983, 165). How to rationally update subjective probabilities in these cases? The standard Bayesian response is that one needs to Jeffrey conditionalize. For illustration: Suppose that a near-sighted agent observes a coin toss without glasses from some distance. She thinks that the coin most likely landed tails, but she is not fully certain. The observation shifts her initial subjective probabilities of the coin landing heads or tails, $\text{Pr}(E)$ and $\text{Pr}(\neg E)$, to their new values $\text{Pr}^*(E)$ and $\text{Pr}^*(\neg E)$, where $\text{Pr}^*(E)$ is greater than $\text{Pr}^*(\neg E)$. In an informal and simplified sense, JC then prescribes the agent to propagate this change to the rest of her beliefs by partially updating them by conditioning on her evidence that the coin landed heads and partially on her evidence that it landed tails.

But why should an agent update by JC and not by some other rule? A common defense of JC is pragmatic. The argument is based on a proof that any agent who does not update by JC is vulnerable to a so-called dynamic Dutch book. In other words, a bookie who knows just as much as the agent (i.e., they both know the agent's belief updating strategy) can offer the agent a series of bets that the agent evaluates as fair but that lead to a guaranteed loss (Armendt, 1980). The converse was also proven: any agent who updates by JC is invulnerable to dynamic Dutch books (Skyrms, 1987).

The argument is convincing. A rational agent must avoid sure losses. However, as the problems identified in this paper show, invulnerability to Dutch books is not a be-all and end-all justification of JC. I show that there exist many situations where JC prescribes the agent to assign an arbitrarily high probability to a false hypothesis after observing specific sequences of uncertain but not „misleading“ evidence. Hence, while JC offers a pragmatic advantage (invulnerability to Dutch books), I argue that this advantage is offset by the epistemic disadvantage – a rational agent ought, after all, not assign high probability to a false hypothesis (given that the evidence is not misleading). The problem is even more worrying because it is (at least in some well-specified cases) robust with respect to the agent's prior probabilities. In other words, even if an agent who updates by JC is initially highly confident of the true hypothesis, there exist such non-misleading sequences of uncertain observations that she will eventually become highly confident of a false hypothesis.

Consider the following scenario for an illustration of how JC prescribes the agent to become highly confident of a false hypothesis: Freya is a Bayesian microbiologist. She updates her beliefs by Bayesian conditionalization or by JC if she is not fully certain of her evidence and the rigidity condition is satisfied. She has identified some bacteria in a sample and correctly believes it may only be of the A or B strain but not both. She knows that both strains have similar biochemical characteristics, except for the characteristic E, which is 75% likely to be present in strain A, and is always present in strain B. Suppose, further, that she believes it is just as likely that she is to encounter strain A or B, so her prior probabilities are 0.5 for both hypotheses. Further, suppose that she is actually observing strain B, so that the characteristic E is always present. She inspects the sample 40 times and always observes E with 70% certainty (e.g., because her instrument is not completely precise, so she is not completely certain about her observations).

It is easy (if a bit lengthy) to verify that after 40 such observations Freya becomes approximately 0.99 certain that her sample contains strain A (the one where the characteristic E is 0.75 likely), and merely 0.01 certain that she is dealing with strain B that she is actually inspecting. Considering that Freya's evidence was always such that she was reasonably certain that E was present (she was constantly 0.7 certain about it), it is problematic that she assigned a very high probability to strain A and a very low probability to strain B. Note that her observations perfectly fit strain B hypothesis – she was, after all, always more certain that E is present in the sample than not.

The scenario is admittedly oversimplified to serve as an actual example from scientific practice. However, it affords a precise analysis of what principles of JC lead to the problem. I also discuss a number of variations of this problem (e.g., the cases where an agent operates with more hypotheses and different likelihoods) and show when and why the formal properties of JC lead an agent to assign very high probability to a false hypothesis despite non-misleading sequences of observations.

References

- Armendt, B. (1980): Is there a Dutch book argument for probability kinematics? *Philosophy of Science* 47(4), 583–588.
Jeffrey, R. (1983): *The Logic of Decision*. Chicago and London: University of Chicago Press.
Skyrms, B. (1987): Dynamic coherence and probability kinematics. *Philosophy of Science* 54(1), 1–20.

Scientific representation: Resituating the similarity account

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According to a popular version of the similarity account of scientific representation, scientists utilize similarity relations (in certain aspects to certain degrees) between models and their target systems for representational purposes (e.g. Godfrey-Smith 2009; Giere 2010; Mäki 2011). However, within philosophy, there is a long tradition of treating resemblance/similarity accounts with high level of suspicion (e.g. Goodman 1981). More recently, the account has been attacked in the context of scientific representation. For instance, it has been argued that similarity has the wrong logical properties to be an account of representation, or that similarity is neither necessary nor sufficient for representation (e.g. Suárez 2003; Frigg and Nguyen 2017). Others have claimed that due to the metaphysical nature of non-material models, i.e. they do not instantiate the spatio-temporal properties of their target systems, similarity is a non-starter because one cannot compare a non-instantiated property with one that is instantiated (e.g. Hughes 1997; Thomson-Jones 2010; Odenbaugh 2015). Furthermore, if the target does not exist then there can be no relation of similarity between the model and its target (Toon 2012). Some have pushed back against the objections, suggesting that the critics might have misconstrued the similarity account in some ways (e.g. Godfrey-Smith 2009; Chakravartty 2010; Mäki 2011; Weisberg 2013). The way I see it, however, there is more work to be done. One reason for that is that while some have adopted a more pragmatist view, they still have not gone far enough. Indeed, many have been satisfied with drawing a distinction between representation and successful (accurate) representation, without providing much details as to how we should cash out that distinction in order to save the similarity account.

My approach is thus different, and is two-fold. First, I give up on the quest of reinstating the similarity account to its former status as the account of representation. Instead, I adopt the notion of representational style (Frigg and Nguyen 2017) where similarity is no longer construed as grounding the notion of representation. In my view, representation is established by an act of stipulation

(Callender and Cohen 2006). However, such an act cannot be an arbitrary stipulation, as number of authors have shown (e.g. Frigg and Nguyen 2017). Rather, I introduce the notion of pragmatically and cognitively constrained stipulation (PCCS), building up on insights of Bolinska (2013) and Knuutila (2017), among others. Pragmatic constraints come from different research goals. Turning a vehicle into a representation is dependent on what the particular aim is (some vehicles are thus more useful than others, some not at all). Cognitive constraints concern our cognitive make-up. Even ‘non-material’ models often, if not always, have various material dimensions that allow us to have cognitive access to their targets to different degrees.

Second, I argue that the critics of similarity have, perhaps, gone too far with their objections, and as a result, they have underestimated the actual value of similarity judgements not only in scientific practice, but also in ordinary human cognition (e.g. in concept formation). My point is not that they have completely dismissed it (they haven’t), but rather, in focusing on criticism, they have not adequately appreciated the importance of similarity judgements. My aim here is to rehabilitate similarity in a way that neutralizes the objections raised against it, and stays faithful to practice.

I suggest that by resituating the similarity account in the aforementioned manner, i.e. as a representational style that might happen to be entertained after the act of PCCS, we can also provide answers to some of the objections. First of all, it straightforwardly dissolves the objection from wrong logical properties because representation is established by a PCCS. Construing similarity as a representational style also answers the charge of non-necessity as well as non-sufficiency of similarity, because similarity is no longer taken to be necessary and/or sufficient for representation. Furthermore, it allows us to downplay the significance of the metaphysical argument. This is because of the pervasive presence of similarity judgements and the apparent ease with which we engage in this cognitive activity. I suggest that the metaphysical argument conflates the possibility of making such judgments with conditions that allow us to form them. It also provides an answer to the objection from non-existing targets because it treats similarity as a success term that is independent of establishing representation.

It is also wrong to think of similarity only in terms of quantitative comparison between properties (as, for instance, Suárez 2003 does). We see the similarity judgments being employed in various forms; from similarity of properties to similarity of patterns, and from similarity of mechanisms to similarity of behavior. I illustrate this on two chosen examples where similarity judgments come into play. The first concerns the quantitative similarity in the context of biometric technology. The other, being a case of qualitative similarity, is exemplified by the kinds of judgments that enter the decision-making of which model organism to use for a particular research task. Similarity judgments also apply to various levels of abstraction, when, for instance, we are faced with describing a very general (abstract) mechanism of how something function, as the case of explaining the phenomenon of ‘tiger bushes’ documents (Bokulich 2014).

All this suggests that it might be possible to fully, and adequately, appreciate the role of similarity without either claiming that it does most of the heavy lifting, or dismissing it prematurely. Indeed, in my view, we can reinstate the similarity account by way of resituating its role.

References

- Bokulich, A. (2014): How the tiger bush got its stripes: ‘How-possibly’ versus ‘how-actually’ model explanations. *The Monist* 97 (3), 321-338.
- Bolinska, A. (2013): Epistemic representation, informativeness and the aim of faithful representation. *Synthese* 190 (2), 219-234.
- Callender, C. – Cohen, J. (2006): There is no special problem about scientific representation. *Theoria* 55 (1), 67-85.
- Chakravartty, A. (2010): Informational versus functional theories of scientific representation. *Synthese* 172 (2), 197-213.

- Frigg, R., Nguyen, J. (2017): Models and representation. In: Magnani, L., Bertolotti, T. (eds.): *Springer handbook of model-based science*. Dordrecht: Springer, 49-102.
- Giere, R. N. (2010): An agent-based conception of models and scientific representation. *Synthese* 172 (2), 269-281.
- Godfrey-Smith, P. (2009): Models and fictions in science. *Philosophical Studies* 143 (1), 101-116.
- Goodman, N. (1981): *Languages of art: An approach to a theory of symbols*. Indianapolis: Hackett.
- Hughes, R. I. G. (1997): Models and representation. *Philosophy of Science* 64 (Supplement. Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers), S325-S336.
- Knuuttila, T. (2017): Imagination extended and embedded: Artifactual versus fictional accounts of models. *Synthese*, doi: 10.1007/s11229-017-1545-2
- Mäki, U. (2011): Models and the locus of their truth. *Synthese* 180 (1), 47-63.
- Odenbaugh, J. (2015): Semblance or similarity? Reflections on Simulation and similarity. *Biology & Philosophy* 30 (2), 277-291.
- Suárez, M. (2003): Scientific representation: Against similarity and isomorphism. *International Studies in the Philosophy of Science* 17 (3), 225-244.
- Thomson-Jones, M. (2010): Missing systems and the face value practice. *Synthese* 172 (2), 283-299.
- Toon, A. (2012): Similarity and scientific representation. *International Studies in the Philosophy of Science* 26 (3), 241-257.
- Weisberg, M. (2013): *Simulation and similarity: Using models to understand the world*. Oxford and New York: Oxford University Press.

B. Philosophy of Natural Science

Physical Causation in General Relativity

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At present, Phil Dowe's Conserved Quantity Theory (CQT) is still one of the most robust physical theories of causation. Dowe conceives causal processes and causal interactions in terms of possession and exchange of conserved quantities, respectively; that is, quantities that are governed by conservation laws. From this characterization of the causal relation, the author develops a well-articulated theory that would satisfy the needs of a physical concept of causation.

Criticisms to the CQT have been multiple and varied. However, in this paper we will focus on a criticism that opens an important line of analysis, which, we believe, would allow us to advance towards a refinement of Dowe's theory. Broadly speaking, this criticism reads as follows. The CQT accounts for causal phenomena in the context of Newtonian mechanics and special relativity with no serious obstacle. However, when the CQT is applied to the field of general relativity (GR), Dowe's proposal shows relevant difficulties, which find their roots in the difficulties to formulate genuine conservation principles in the framework of GR (see, e.g., Rueger 1998, Hoefer 2000, Curiel 2000, Vicente 2002, Luper 2009, Lam 2010). To these criticisms, Dowe answers that the identity between 'causal process' and 'world line of an object that possesses a conserved quantity' is contingent, and not metaphysically necessary. Therefore, the fact that there are general relativistic spacetimes in which global conservation laws do not hold does not entail that global conservation laws fail in our world.

The main issue addressed in this talk is, then, the serious drawbacks of the CQT to capture the essence of causation in general relativistic contexts. We will argue that, in order to approach a solution to this problem, it is necessary to accomplish a detailed analysis of the following subproblems:

(1) Deciding whether, with his theory, Dowe intends to offer a precise definition or rather a mere characterization of causal processes and interactions. Clarifying this point is the clue to understand whether the author offers a genuine theory of physical causation or a mere description of what causal relations are in our physical world, but taking the extension of the concept of physical causation delimited by further criteria.

(2) Determining whether physical laws (of classical mechanics, special relativity, general relativity, and so on) are the fundamental principles from which the respective conservation laws must be derived, or, on the contrary, it is possible to conceive conservation laws as principles of wider scope or more fundamental than physical laws with intertheoretical validity.

(3) Clarifying the notion of physical possibility. According to an extended view in philosophy of physics, a state of affairs is physically possible if it satisfies the physical laws of the actual world; from this perspective, what is physically possible is determined by the laws of a given theory. However, this is not the notion of physical possibility that prevails in scientific practice: very often, factors beyond the laws of the theory play a relevant role in the discrimination between the physically possible and the physically impossible states of affairs. Conservation principles can be counted among those factors. Depending on the status assigned to the conservation principles, as discussed in the previous point, they may act or not as an additional criterion to constrain the set of physically possible worlds that can be obtained from the dynamic laws of a physical theory.

It will be argued that the adequate development and analysis of each of the above problems allows us to take a definite position on whether it is possible, or not, to retain Dowe's notion of causation in GR and, with this, gives the elements to clarify the true scope of CQT. Besides this, we consider that this discussion is the first step towards a theory of physical causation even riper than that proposed by Dowe.

In summary, in this presentation we intend to contribute with some clarifications and/or precisions that will allow us to elucidate the conditions that must be satisfied for a correct application of CQT in the context of the GR. This analysis will allow us to have a better understanding of the range of application of the CQT. The final aim is that the discussions of this problem supply some clues for the development of a theory of physical causation more adequate than the CQT for physics.

References

- Beebee, H., Hitchcock, C., Menzies, P. (2009): *Causation*. New York: Oxford University Press.
- Bigelow, J., Pargetter, R. (1990a): Metaphysics of Causation. *Erkenntnis*, 33: 89-119.
- Brading, K., Castellani, E. (2003): *Symmetries in Physics. Philosophical Reflections*. Cambridge: Cambridge University Press.
- Chakravartty, A. (2005): Causal Realism: Events and Processes. *Erkenntnis*, 63: 7-31.
- Choi, S. (2003): The Conserved Quantity Theory of Causation and Closed Systems. *Philosophy of Science*, 70: 510-530.
- Curiel, E. (2000): The Constraints General Relativity Places on Physicalist Accounts of Causality. *Theoria*, 15: 33-58.
- Deshmukh, P. C., Venkataraman, S. (2011): Obtaining Conservation Principles from Laws of Nature. *Bulletin of Indian Association of Physics Teachers*, 3: 143-148.
- Dowe, P. (1992a): Process Causality and Asymmetry. *Erkenntnis*, 37: 179-196.
- Dowe, P. (1992b): Wesley Salmon's Process Theory of Causality and the Conserved Quantity Theory. *Philosophy of Science*, 59: 195-216.
- Dowe, P. (1993): On the Reduction of Process Causality to Statical Relations. *British Journal for the Philosophy of Science*, 44: 325-327.
- Dowe, P. (1995a): Causality and conserved quantities: a reply to Salmon. *Philosophy of Science*, 62: 321-333.
- Dowe, P. (1995b): What's Right and What's Wrong with Transference Theories. *Erkenntnis*, 42: 363-374.
- Dowe, P. (2000): *Physical Causation*. Cambridge: Cambridge University Press.
- Dowe, P. (2000a): The Conserved Quantity Theory Defended. *Theoria*, 15: 11-31.
- Ducasse, C. (1969): *Causation and the Types of Necessity*. New York: Dover.
- Earman, J. (1986): *A Primer on Determinism*. Dordrecht: Reidel.
- Hoefer, C. (2000): Energy Conservation in GTR. *Studies in History and Philosophy of Modern Physics*, 31: 187-199.
- Lam, V. (2010): Metaphysics of Causation and Physics of General Relativity. *Humana.Mente*, 13: 61-80.
- Lange, M. (2007): Laws and Meta-laws of Nature: Conservation Laws and Symmetries. *Studies in History and Philosophy of Modern Physics*, 38: 457-481.
- Lange, M. (2011): Conservation Laws in Scientific Explanations: Constraints or Coincidences?. *Philosophy of Science*, 78: 333-352.
- Lupher, T. (2009): A Physical Critique of Physical Causation. *Synthese*, 167: 67-80.
- Rueger, A. (1998): Local Theories of Causation and the A Posteriori Identification of the Causal Relation. *Erkenntnis*, 48: 25-38.
- Salmon, W (1984): *Scientific Explanation and the Causal Structure of the World*. Princeton: Princeton University Press.
- Salmon, W. (1997): Causality and Explanation: A Reply Two Critiques. *Philosophy of Science*, 64: 461-477.
- Tooley, M. (1987): *Causation: A Realist Approach*. Oxford: Clarendon.
- Van Fraassen, B. (1989): *Laws and Symmetry*. Oxford: Clarendon.
- Vicente, A. (2002): The Localism of the Conserved Quantity Theory. *Theoria*, 45: 563-571.
- Vicente, A. (2005): La Teoría CQ y el Fisicismo. *Enrahonar*, 31: 203-211.

The problem of the 3N dimensions in Quantum Mechanics: a chemical approach

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The wave function is a central element in quantum mechanics, since it represents the state of the system and participates in the dynamics of the system through its evolution according to the Schrödinger equation. However, even today, almost a hundred years after the advent of quantum mechanics, the meaning of the wave function remains the subject of debate. In this context, the problem of the 3N dimensions of the wave function is of particular interest in the philosophy of physics. In fact, the debates around the issue have an important impact on the way in which we conceive the world around us. This is clearly manifested by the intense discussions that have taken place in recent years (cfr. Monton 2006; Ney & Albert 2013).

From his early work, Schrödinger tried to endow the wave function with a physical meaning, first as a kind of vibration in the atom (Schrödinger 1926a) and later as a tool for obtaining the electron density (Schrödinger 1926b). With these proposals, Schrödinger intended to develop an ontology of the wave function in a space of three dimensions, in agreement with the world in which we live. However, some years later he was disappointed with these proposals.

In recent years, different positions have arisen regarding the dimensionality of wave function. On the one hand, it is possible to propose an ontology in which the wave function is the most important aspect of quantum theory, so that its mathematical nature is directly related to “reality” (eg. Albert 2013). In spite of the fact we only perceive three dimensions, the authors who advocate for this position support a vision in which the real physical space has actually 3N dimensions. This position is commonly called “wave function realism”. On the other hand, a different position conceives the wave function as a mere mathematical artifact belonging to the formalism of quantum mechanics. The defenders of this view usually postulate a “primitive ontology” with a real space having only three dimensions (eg. Monton 2013, Allori 2013). Finally, there is a third position that tries to reconcile the two previous ones. It proposes an ontological picture in which both the space of 3N dimensions and that of three dimensions coexist (eg. Monton 2006). This discussion is still relevant in philosophy of physics and even in physics too.

In this work we will introduce a new perspective, coming from chemistry. In the field of quantum chemistry, the question about the 3N dimensions has not been discussed as deeply as in the context of quantum mechanics. From a pragmatic standpoint, chemists are faced with entities that exist in real space in their daily work; hence there is no discussion about the dimensional nature of wave function: rather, it is natural to try to turn it into a three-dimensional entity. In the context of quantum chemistry, we use the so called orbital approximation, which allows us to write the total wave function of a system as a product of mono-electron wave functions (Atkins & de Paula 2006). Under this approximation, the wave function of a given electron depends only on the variables of this electron; therefore, it evolves in the space of three dimensions (Lowe & Peterson 2006). We will analyze the procedure by means of which quantum chemists use this approach, in order to suggest a

possible solution to the problem of dimensionality of the wave function. In particular, we will argue that it is possible to formalize the procedure performed by chemists when they use the orbital approximation, as the result of the application of two mathematical operations: first a projection in the Hilbert space, and then a change of variables. With the help of this formalization we can go beyond the approximation itself and propose a valid argument for the ontology of quantum chemistry. In addition to this, we will argue that the main purpose behind the application of the orbital approximation is associated with conceptual arguments usually used in quantum chemistry.

References

- Albert, D. (2013): Wave Function Realism. In: Ney, A., Albert, D. Z. (eds.): *The Wave Function*. New York: Oxford University Press.
- Allori, V. (2013): Primitive Ontology and the Structure of Fundamental Physical Theories. In: Ney, A., Albert, D. Z. (eds.): *The Wave Function*. New York: Oxford University Press.
- Atkins, de Paula (2006): *Physical Chemistry*. New York: Oxford University Press.
- Lowe, J. P., Peterson, K. A. (2006): *Quantum Chemistry*. Burlington, San Diego and London: Elsevier Academic Press
- Monton, B. (2006): Quantum Mechanics and 3N-Dimensional Space. *Philosophy of Science*, 73: 778-789.
- Monton, B. (2013): Against 3N-Dimensional Space. In: Ney, A., Albert, D. Z. (eds.): *The Wave Function*. New York: Oxford University Press.
- Schrödinger, E. (1926a): Quantisierung als eigenwertproblem (Erste mitteilung). *Annalen der Physik*, 79: 361-376. English traslation in E. Schrödinger (author): *Collected Papers on Wave Mechanics* 1928. London and Glasgow: Blackie & Son Limited.
- Schrödinger, E. (1926b): Quantisierung als eigenwertproblem (Zweite mitteilung). *Annalen der Physik*, 79: 489-527. English traslation in E. Schrödinger (author): *Collected Papers on Wave Mechanics* 1928. London and Glasgow: Blackie & Son Limited.

The explanatory role of cellular automata models in biology

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The causation-based analysis of the notion of explanation in sciences has dominated the contemporary philosophical literature on explanation in the last three decades (e.g., Salmon (1984), Strevens (2004)). This analysis suggests that scientific explanation (of the empirical phenomena) acquires its explanatory power in virtue of tracking some causal relations in the world. Contrary to this analysis, some have identified a different class of explanations of the empirical phenomena in which the explanandum is explained by an appeal to the truth of a (set of) mathematical statement(s) or by an appeal to a set of mathematical models. This class of scientific explanations is non-causal (e.g., Lange (2012), Huneman (2010), Batterman and Rice (2014), Colyvan (2014)).

In this paper, first, I look into a particular kind of mathematical models, cellular automata models, that haven't been discussed in the philosophical literature on explanation. These models generate some biological patterns at the macro-level of the phenomena. The biological pattern under discussion is the formation of skin color. Cellular automata models can be susceptible to provide genuine non-causal explanations. In what follows, I assess whether generating the biological pattern in terms of a cellular automata model is explanatorily relevant. By providing a detailed case study from developmental biology, I argue that the generation of the cellular automata pattern is not on its own

genuine non-causal explanation of the formation of skin color patterns. Rather, the cellular automata model is the emergent result of the combination of two components: (i) Turing causal explanations at the micro-level skin scales and (ii) some (purely) mathematical derivations (due to the techniques of computational geometry) from the Turing equations given the empirical conditions of skin thickness variations. In other words, cellular automata models give us information about the dynamics of the computational structure underlying the empirically observed biological processes of the formation of skin color patterns. I discuss that these cellular automata models are partial contributors to a comprehensive explanation of the formation of skin color patterns.

Second, I analyze the widely discussed micro-level mechanistic explanation of the formation of skin color patterns by an appeal to Turing equations for the skin colored cells. These equations are based on the diffusion-reaction principle and have been traditionally a paradigmatic approach to explain the development of the formation of skin color pattern in animals. I argue that Turing equations at the micro-level along with some (purely) mathematical derivations using techniques of computational geometry explain the emergence of cellular automata as the pattern generator on a macro-level, at the mesoscopic skin scale. Given that, I argue that the comprehensive explanation of the formation of the skin color pattern has a hybrid nature and has both the causal and non-causal components. In other words, the comprehensive explanation for why the skin color of an ocellated lizard has a cellular automata pattern at the mesoscopic skin scale? invokes a hybrid explanation: micro-level causal mechanisms along with an implementation of some mathematical derivation techniques that explain the emergence of the cellular automata model of a biological process. In other words, the emergence of the discrete generative cellular automaton (in mesoscopic skin scale) as the underlying formal structure of the color pattern formation is explained by continuous Turing mechanisms and mathematical derivations from such equations, under certain empirical circumstances (i.e., skin thickness variations).

The paper proceeds as follows. In Section 2, I introduce two of the most well-known accounts of mathematical explanations of the empirical phenomena. In Section 3, I provide a brief overview of the main principles of cellular automata and I present a cellular automaton model of the formation of skin color patterns in a species of reptiles. In Section 4, I discuss Turing explanations for the emergence of the cellular automaton at the mesoscopic skin scale and I sketch an explanatory schema for how the mechanistic explanation of the emergence of the cellular automaton model explains the formation of the skin color patterns on the ocellated lizard. I close the paper by sketching a hybrid account of explanation which invokes both the genuine causal and non-causal components.

References

- Batterman, R. W., C. C. Rice (2014): Minimal model explanations. *Philosophy of Science* 81 (3), 349-376.
- Colyvan, M. (2014): The undeniable effectiveness of mathematics in the special sciences. In: *New directions in the philosophy of science*, pp. 63-73. Springer.
- Huneman, P. (2010): Topological explanations and robustness in biological sciences. *Synthese* 177 (2), 213-245.
- Lange, M. (2012): What makes a scientific explanation distinctively mathematical? *The British Journal for the Philosophy of Science* 64 (3), 485-511.
- Salmon, W. (1984): *Scientific explanation and the causal structure of the world*.
- Strevens, M. (2004): The causal and unification approaches to explanation unified | causally. *Noûs* 38 (1), 154-176.

Getting physical possibility straight: what makes an event physically possible?

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There is a broadly-assumed viewpoint in philosophy of science and philosophy of physics supporting the idea that physical possibility is determined by what is possible according to the physical laws that obtain in the actual world. Since, in philosophy, a possible world is a collection of possible events representing possible but alternative stories to the actual world, a physically possible world is any possible world in which natural laws that obtain in the actual world are satisfied.

This view allows introducing more rigorously the modal parlance in physics and to differentiate the notion of logical and metaphysical possibility from physical possibility. John Earman, in his famous *A Primer on Determinism*, has promoted this conventional wisdom on the matter in arguing that an event is physically possible if and only if it is allowed by actual world's physical laws. Accordingly, an event is said to be physically possible if and only if there is a physically possible world in which such event may occur. One of the strongest reasons for favoring this view is that physical laws play an essential role in scientific explanation and modal generalizations. Physical possibility, theoretical explanation and physical laws seem to go hand-in-hand to the extent that the physical content of a theory is outwardly determined by the set of possible worlds wherein the theory is true, to wit, wherein its physical laws hold.

Notwithstanding this widely-held viewpoint, there are relevant contexts in which physical possibility has been understood a bit differently. As well known, certain events or explanations are discarded by the physics community for being physically impossible though keeping with laws of nature. Frequently, factual or material circumstances (as a particular setting or boundary conditions) are invoked for establishing that an event is physically impossible despite conforming to the theory's laws. In other cases, an event or solution are regarded as physically impossible due to violation of metaphysical principles. For instance, some solutions (that is, models of the universe) of Einstein's field equation are not seriously taken to be physically possible as they involve closed timelike curves that might violate the principle of causality. Finally, purely mathematical considerations are sometimes also brought up: solutions featuring wormholes or white holes are discarded as physically possible solutions as they are mathematically unstable. Overall, physicists' discourse is pledged of extra-nomic criteria to speak about what is physically possible and what is not.

In this presentation we shall show that the conventional viewpoint on physical possibility turns out to be inadequate to sharply capture the notion of physically possible as conceived in scientific practice. As mentioned above, we shall argue that physicists typically invoke various extra-nomic criteria when determining the physically possible, taking examples from general relativity. Even though such criteria are not always clear or explicit in the scientific discourse, the point we want to make is that they matter in something like the way physical laws are generally supposed to matter: they favor certain generalization and forbid others, and they also play an essential role in explanation of phenomena. By involving such criteria, we hope not only to reach a far-reaching, practice-based understanding of physical possibility, but also to get a sharper notion of what is the theoretical content of a physical theory.

Non-individuals and Structural Reconceptualization of Objects in Spacetime Structuralism

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Spacetime Structuralism can be considered as a family of positions concerned with interpreting spacetime as described by the currently most successful theory of gravitation, General Theory of Relativity (GR). Motivations for such enquiries are situated e.g. in problems about validity of the debate between substantivalisms and relationalisms about the ontological status of spacetime, where the terms of the debate are explicated in close analogy to the classical debate between Leibniz and Clarke about space. There are many takes on how to interpret spacetime in structural terms. Brands of structural realism in the context of spacetime physics which I want to analyze are those developed by M. Esfeld/V. Lam and D. Rickles.

Esfeld's and Lam's Moderate Ontic Structural Realism (MOSR) is a non-eliminative structural realism. This amounts to joining proponents of Ontic Structural Realism (OSR) in their denial of non-qualitative facts about objects and of intrinsic properties while retaining objects as non-individuals. This means that there is no primitive individuation of objects. Instead, identity conditions are to be described in relational terms. Furthermore, in MOSR one interprets objects as ontologically on a par with relations. Numerical distinctiveness of spacetime points is stipulated, but their identity is fully dependent on the web of spacetime relations. MOSR is similar to views called sophisticated substantivalism. Proponents of those views precisely show how spacetime points can be cashed out as non-individuals in physico-mathematical considerations.

S. French claimed that postulating the existence of non-individuals does not suit well the structuralist agenda since non-individuals can be conceived as some kind of particulars. For we are stipulating the existence of non-individual objects, and French deems this as an arbitrary move based on set-theoretical orthodoxy and incorrect usage of the principle of the identity of indiscernibles. French proposed to reconceptualize objects entirely in structural terms. In the context of fundamental physics this means to cash out objects as representational devices introduced in mathematical treatments of symmetries. Symmetries are understood here as constituting the ontological content of fundamental theories in physics. A plausible example of how this can make sense in the context of spacetime physics was presented by D. Rickles. He arrived at the conclusion that purely structurally conceived ontological assumptions about spacetime, relative to GR, are exhausted in the observables of GR with respect to relevant symmetrical transformations supported within this theory.

In my presentation I would like to argue that there is a genuine competition between the concept of spacetime points as non-individuals and the concept of observables, when it comes to accounting for spacetime ontology in structural terms. Then I would like to shortly discuss pros and cons of both concepts. Finally, I would like to argue that the "non-individuals" view can accommodate the advantage of the "observables" view in reducing redundant metaphysical claims about modal facts concerning spacetime points. I shall argue that spacetime points considered as non-individuals can be analyzed as "relationalists", as F.A. Muller dubs it. Then I want to argue, using P.M. Ainsworth's general classification of possible structural realisms, that one can add to an ontology of spacetime points (as non-individuals) such predicates that correspond to Rickles' view about observables.

References

- Ainsworth, P.M. (2010): What is ontic structural realism?. *Studies in History and Philosophy of Modern Physics* 41, pp. 50–57.
Bain, J. (2006): Spacetime Structuralism. In: Dieks, D. (ed.): *The Ontology of Spacetime Volume 1*,

- Elsevier, Amsterdam, pp. 37-66.
- Chakravartty, A. (2003): The Structuralist Conception of Objects. *Philosophy of Science* 70, pp. 867 – 878.
- Dorato, M., (2000): Substantivalism, Relationism, and Structural Spacetime Realism. *Foundations of Physics* 30, pp. 1605 – 1628.
- Dorato, M. (2008): Is Structural Spacetime Realism Relationism in Disguise? The Supererogatory Nature of the Substantivalism/Relationism Debate. In: Dieks, D. (ed.), *The Ontology of Spacetime Volume 2 of Philosophy and Foundations of Physics*, Elsevier, Amsterdam, pp.17 – 38.
- Earman, J., Norton, J.D. (1987): What Price Spacetime Substantivalism, British Journal for the Philosophy of Science 38, pp. 515 – 525.
- Esfeld, M., Lam, V. (2008): Moderate structural realism about space-time. *Synthese* 160, pp. 27 – 46.
- Esfeld, M., Lam, V. (2012): The Structural Metaphysics of Quantum Theory and General Relativity. *Journal for General Philosophy of Science* 43, pp. 243 – 258.
- French, S. (2014): *The Structure of The World*. Metaphysics and Representation. Oxford University Press, Oxford.
- Greaves, H. (2011): In search of (spacetime) structuralism. *Philosophical Perspectives* 25, pp.189 – 204.
- Müller, F.: How to Defeat Wüthrich's Abysmal Embarrassment Argument against Space-Time Structuralism. *Philosophy of Science* 78, pp. 1046-1057.
- Rickles, D. (2008): *Symmetry, Structure, and Spacetime*. Elsevier, Amsterdam.
- Stachel, J. (2014): The Hole Argument and Some Physical and Philosophical Implications. *Living Rev. Relativity* 17, <http://www.livingreviews.org/lrr-2014-1>.
- Wald, R.M. (1984): *General Relativity*. University of Chicago Pres, Chicago.
- Wüthrich, Ch. (2009): Challenging the Spacetime Structuralist. *Philosophy of Science* 76 (2009), pp. 1039 – 1051.

Acknowledgements

Damian Luty is Adam Mickiewicz University Foundation scholar in 2017/2018 academic year. This presentation is a part of a project funded by National Science Centre in Poland, grant registration number: 2016/23/N/HS1/00531

Functional Integration in the Endosymbiotic Origin of Mitochondria

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Functional integration is broadly defined in life sciences as the causal interdependence among the subsystems forming an organism. Since the concept of ‘functional integration’ is based on a common sense (physiological) view of organisms, it appears vague and unable to define degrees of biological individuality (Pradeau 2010). In other words, the (physiological) view of functional integration does not specify in which sense a biological individual (from the simplest prokaryotes to the most complex multicellular eukaryotes) is ‘functionally integrated’. Although functional integration plays an important role in most of functional explanations, neither systemic (Cummins 1975; Craver 2001; Davies 2001), nor etiological (Wright 1973; Millikan 1984; Neander 1991), nor dispositional (Bigelow and Pargetter 1987) approaches to biological functions have taken it into account. The organizational perspective, by contrast, interprets functional integration as the mutual dependence of the constitutive constraints that collectively maintain the whole biological organization by allowing it to exhibit biological individuality (Moreno and Mossio 2015).

It is highly debated whether functional integration is an important requirement for defining the biological individuality of symbiotic organisms (e.g., holobionts), because the mutual dependence among the functions of different organisms in many cases does not lead to an ‘integrated’ individual (Skillings 2016; Queller and Strassmann 2016). The purpose of this paper is to investigate how the endosymbiotic relationship between the proto-mitochondrion and a proto-eukaryotic cell has led to a more integrated biological organization and a new biological individual (i.e. the eukaryotic cell) by means of a functional redefinition of both the endosymbiont and the host. Two theoretical questions will be addressed: first, how did the endosymbiont and the host achieve a functionally integrated organization?; second, what were its evolutionary consequences?

These questions will be discussed by adopting an organizational approach, according to which the analysis of both structural and physico-chemical conditions of biological phenomena can shed some light on the organization of living beings. The functional redefinition of the bioenergetic systems of proto-mitochondrion and proto-eukaryote will be examined, because they seem to have played a pivotal role in the emergence of a more functionally integrated organization of the eukaryotic cell. In particular, three phenomena will be analysed: first, the selective loss of biochemical pathways both in the endosymbiont and in the host (Gabaldón and Huynen 2007; Martin et al. 2015); second, the appearance of the translocase of inner membrane (TIM) and outer membrane (TOM) of the mitochondrion (Cavalier-Smith 2006, 2007; Dolezal et al. 2006); finally, the control of the redox poise of the electron transport chain (Allen 1993; Allen and Raven 1996; Lane 2005, 2007, 2015).

These three phenomena suggest that the functional redefinition of bioenergetic systems contributed to not only the metabolic codependence between the host and the endosymbiont, but also a dramatic transformation of both organisms that led to a new biological individual (i.e. the eukaryotic cell). Thus, the functional redefinition of the systems involved in energy production was a key factor for the functional integration between a proto-mitochondrion and a proto-eukaryotic cell.

It will be argued that, in the case of eukaryogenesis, the concept of ‘functional integration’ is intimately connected with those of ‘biological novelty’ and ‘biological individuality’, insofar as the emergence of a more integrated symbiotic organization has led, by means of functional redefinition of the host and the endosymbiont, to new biological functions and a new biological structure exhibiting a specific kind of individuality.

References

- Allen, J. F. (1993): Control of Gene Expression by Redox Potential and the Requirement for Chloroplast and Mitochondrial Genomes. *Journal of Theoretical Biology*, 165: 609-631.
- Allen, J. F., Raven, JA (1996): Free-radical-induced mutation vs redox regulation: costs and benefits of genes in organelles. *Journal of Molecular Evolution*, 42: 482-492.
- Bigelow, J., Pargetter, R. (1987): Functions. *The Journal of Philosophy*, 84: 181-196.
- Cavalier-Smith, T. (2006): Origin of mitochondria by intracellular enslavement of a photosynthetic purple bacterium. *Proceedings of the Royal Society B*, 273: 1943-1952.
- Cavalier-Smith, T. (2007): The Chimaeric Origin of Mitochondria: Photosynthetic Cell Enslavement, Gene-Transfer Pressure, and Compartmentation Efficiency. In: Martin, W. F., Müller, M. (eds.): *Origin of Mitochondria and Hydrogenosomes* (pp. 160-199): Heidelberg: Springer.
- Craver, C. F. (2001): Role Functions, Mechanisms, and Hierarchy. *Philosophy of Science*, 68: 53-74.
- Cummins, R. (1975): Functional Analysis. *The Journal of Philosophy*, 72: 741-765.
- Davies, P. S. (2001): *Norms of Nature: Naturalism and the Nature of Functions*. Cambridge (Mass.): MIT Press.
- Dolezal, P., Likic, V., Tachezy, J., Lithgow, T. (2006): Evolution of the Molecular Machines for Protein Import into Mitochondria. *Science*, 313: 314-318.
- Gabaldón, T., Huynen, M. A. (2007): From Endosymbiont to Host-Controlled Organelle: the Hijacking of Mitochondrial Protein Synthesis and Metabolism. *Plos Computational Biology*, 3: 2209-2218.

- Lane, N. (2005): *Power, sex, suicide: mitochondria and the meaning of life*. Oxford: Oxford University Press.
- Lane N. (2007): Mitochondria: Key to Complexity. In: Martin, W. F., Müller, M. (eds.): *Origin of Mitochondria and Hydrogenosomes* (pp. 12-38): Heidelberg: Springer.
- Lane, N. (2015): *The Vital Question: Why is Life the Way it is?*. London: Profile Books LTD.
- Martin W. F., Garg, S., Zimorski, V. (2015): Endosymbiotic theories for eukaryote origin. *Philosophical Transactions B*, 370: 1-18.
- Millikan, R. G. (1984): *Language, thought, and other biological categories*. Cambridge (Mass.): MIT Press.
- Moreno, A., Mossio, M. (2015): *Biological Autonomy. A Philosophical and Theoretical Enquiry*. Dordrecht: Springer.
- Neander, K. (1991): Functions as Selected Effects: The Conceptual Analyst's Defense. *Philosophy of Science*, 58: 168-184.
- Pradeau, T. (2010): What is An Organism? An Immunological Answer. *History and Philosophy of the Life Sciences*, 32: 247-268.
- Queller, D. C., Strassmann, J. E. (2016): Problems of multi-species organisms: endosymbionts to holobionts. *Biology and Philosophy*, 31: 855-873.
- Skillings, D. (2016): Holobionts and the ecology of organisms: Multi-species communities or integrated individuals?. *Biology and Philosophy*, 31: 875-892.
- Wright, L. (1973): Functions. *The Philosophical Review*, 82: 139-168.

Biological information – what was the problem again?

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The idea that genes carry information for development and that this information is transmitted across generations is so well established in the biological discourse that has even made its way into common sense. The notion of information accompanied the rise of molecular biology and remains part of the fundamental language for describing and explaining the nature of DNA and various genetic mechanisms. However, explicating the informational talk in biology has proven to be a puzzling task and all sorts of view have been defended both by philosophers and biologists.

Even though the debate around information in biology cannot be ascribed a long philosophical history, the literature on the topic exhibits interesting divergence. The divergence we have in mind is not that of different stances towards one problem (which is to be expected), but divergence in the very way the problem is presented and what it is considered to be at stake. For some authors, the problem arises from -alleged- preformationist and genetic deterministic implications of the idea that genes carry information for development and, relatedly, that such way of describing genetic material and genetic processes endows DNA with a sort of causal primacy that has been claimed not to be empirically supported (Oyama 1985). Thus, in one view, the relevant question is whether (and to what extent) are genes special causes in development, the notion of 'information' being just one way (among others) to express such a privilege (Oyama 2000, Waters 2007, Austin 2015, Weber forthcoming). Others approach the debate in terms of literal versus metaphorical use of language, in which case the question

is that of the cognitive and scientific value of metaphors, in one case, or about the precise literal meaning of the term (Fox Keller 2003, Levy 2011). In turn, defenses of the literality of information in biology come in many different flavors. Some philosophers, for example, take the issues to be whether or not semantic properties are legitimately attributable to biochemical entities, what is an appropriate semantic framework, or what properties are semantic in nature (Stegmann 2005, Shea 2007). Others take the literal meaning of biological information to be rooted in causation, so that the philosophical task is to analyze the peculiarities of genetic causation (Weber 2006, Stotz & Griffiths 2017). Others regard information as a matter of transmission and wonder whether the concept of information can be the conceptual key for theorizing about the different inheritance systems (Jablonka 2002). Yet, others believe that the nature of the informational language in molecular biology is mechanistic, and articulate it more explicitly from a mechanistic conceptual framework (Bogen & Machamer 2011, Kjosavik 2014).

Things get even messier and more complex due to the critical fact that ‘information’ is said in many ways. There is no univocal sense of information, in biology or elsewhere - a fact well known in other areas (Lombardi 2004, Floridi 2017). For example, different interpretations of Shannon’s theory of information are widely discussed in philosophy of physics. Moreover, information now stars its own field of philosophical inquiry: philosophy of information (Floridi 2011). Contributions to the philosophical aspects of information beyond biology have not echoed the philosophy of biology as would be expected.

As a consequence of this scattered philosophical landscape, one gets the impression that philosophers of biology are proffering parallel independent monologs (or family of monologs) with scarce common concerns, rather than addressing the same one issue (or same one coherent set of issues). This is apparent when one attempts to elaborate a taxonomy of the views and approaches to biological information: however one chooses to classify and systematize them, it feels largely artificial. A direct and serious consequence of such state of affairs is the difficulties for evaluating the various philosophical contributions, since this is possible only with reference to a well-defined problem. Thus, every once in a while, time calls for a step-back to reconsider the problems themselves.

Here, we will focus on the notion of information in biology with a twofold aim. On the one hand, we will offer a reconstruction of the problem(s) around the concept of information in biology. In order to motivate our reconstruction, we will draw from various accounts available in the philosophy of biology literature, but, importantly, we will also argue for the need of a broader and more comprehensive perspective and turn to discussions around the concept of information beyond biological contexts. On the other hand, our analysis will yield a set of adequacy criteria that any philosophically sound account of biological information should meet and in reference to which the various available accounts of biological information should be evaluated.

References

- Adriaans, P. (2013): Information. Zalta, E. N. (ed.): *The Stanford Encyclopedia of Philosophy*
URL = <<https://plato.stanford.edu/archives/fall2013/entries/information/>>.
- Austin, C. (2015): The dispositional genome: primus inter pares. *Biology & Philosophy*, 30, 2, pp. 227–246.
- Bogen, J., Machamer, P. (2011): Mechanistic Information and Causal Continuity. In: Illari, P., Russo, F., Williamson, J. (eds.): *Causality in the Sciences*. Oxford University Press, 845-864.
- Floridi, L. (2011): *The Philosophy of information*. Oxford; Oxford University Press.
- Floridi, L. (2017): Semantic Conceptions of Information. Zalta, E. N. (ed.): *The Stanford Encyclopedia of Philosophy*,
URL = <<https://plato.stanford.edu/archives/spr2017/entries/information-semantic/>>.
- Jablonka, E. (2002): Information: Its Interpretation, Its Inheritance and Its Sharing. *Philosophy of Science*, 69, 578-605.

- Keller, E. F. (2003): *Making Sense of Life. Explaining Biological Development with Models, Metaphors, and Machines*. Cambridge: Harvard University Press.
- Kjosavik, F. (2014): Genes, Structuring powers and the Flow of Information in Living Systems. *Biology and Philosophy*, 29 (3), 379-394.
- Levy, A. (2011): Information in Biology: A Fictionalist Account. *Noûs*, 45(4): 640-657.
- Lombardi, O. (2004): What is information?. *Foundations of Science*, 9, pp. 105–134.
- Oyama, S. (1985): *The Ontogeny of Information: Developmental Systems and Evolution*. Cambridge: Cambridge University Press.
- Oyama, S. (2000): Causal democracy and causal contributions in Developmental Systems Theory. *Philosophy of Science*, 67, S332-S347.
- Shea, N. (2007): Representation in the Genome and in other Inheritance Systems. *Biology and Philosophy*, 22, 313–331.
- Stegmann, U. (2005): Genetic Information as Instructional Content. *Philosophy of Science*, 72, 3: 425–443.
- Stotz, K., Griffiths, P. E. (2017): Biological Information, causality and specificity – an intimate relationship. In: Walker, S. I., Davies, P., Ellis, G. (eds.): *From Matter to Life: Information and Causality*. Cambridge: Cambridge University Press, pp. 366–390.
- Waters, C. K. (2007): Causes that make a difference. *Journal of Philosophy*, 104, pp. 551–579.
- Weber, M. (Forthcoming): Causal Selection versus Causal Parity in Biology: Relevant Counterfactuals and Biologically Normal Interventions. In: Waters, K., Travisano, M., Woodward, J. (eds.): *Philosophical Perspectives on Causal Reasoning in Biology*. Minneapolis: University of Minnesota Press.
- Weber, M. (2006): The Central Dogma as a Thesis of Causal Specificity. *History and Philosophy of the Life Sciences*, 28, 595-610.

Argumentative structures in biology: a study of pathogen discoveries

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In biology, generally speaking, consensus about the results is not fast and reliable. The time needed for reaching it is much longer than in e.g. high energy physics. And it is often difficult to find a coherent set of inductive rules governing the research in biology. There are various reasons why results in biology are, in general, not so quickly agreed upon and reliable. First, some results that cannot be replicated are published in journals with high impact factors and get a high number of citations (Pusztai et al. 2013). Second, there are deliberately faked results because of the inefficient system of paper retraction and individual career benefits from publishing incorrect data. Third, there may be a problem deciding what constitutes sufficient evidence for a hypothesis, especially if the hypothesis is non-parsimonious, i.e. when the hypothesis is not the simplest explanation of the phenomenon. Fourth, an expectancy bias appears in reports on the results. These factors negatively influence the replicability of biological experiments and slow down consensus (Goodman et al. 2016). For these reasons, we argue for the use of argumentative schemes in order to understand the development of non-parsimonious in pathogenesis.

Specifically, in the talk, two Nobel Prize winning discoveries of disease-causing agents are formally represented: protease-resistant proteins (PrP, i.e. prions) and Helicobacter pylori. These breakthrough results in life science were more complex in comparison to prior beliefs about disease-causing agents. In the prion case, through analysing the number of argumentative steps, we assess the

expectancy bias towards general hypothesis. Moreover, by comparing the argumentative structure of the prion argument to the structure of the general one, we investigate its lack of generality. After argument reconstruction, we point out that, in the case of *Helicobacter pylori*, important evidence, suggesting that traces of bacteria were noticed in the stomach, was neglected. Our formal argument reconstruction serves for the evaluation of similar results. The formal argumentation could be helpful for better understanding of the complex interplay of multifactorial causes of neurodegenerative diseases such as Parkinson's and Alzheimer's disease. We argue that the pursuit of diverse hypothesis is epistemically beneficially from the perspective of the scientific field as a whole.

Though it is reasonable to expect that unexpected discoveries require more testing before being accepted, the simplicity of a hypothesis is not equally epistemically beneficial in every research field. For instance, the application of the parsimony criterion to philosophical questions was criticised by Huemer (2009), because it is often unclear which philosophical view is more complex, using as examples the nominalism/realism and physicalism/dualism debate. Longino (1996) argues in favour of heterogeneity in economics, because it is beneficial for feminist epistemology. While phylogenetics, a subfield of biology, can be regarded as theory with a high degree of uniformity, pathogenesis does not experience the same degree of regular behaviour. The presented examples support this claim. Moreover, disease causes can often be cofactorial and multifactorial. From the perspective of the scientific community working on pathogenesis it is epistemically beneficial to pursue diverse and complex hypothesis.

References

- Goodman, S.N., Fanelli, D., Ioannidis, J.P.A. (2016): What does research reproducibility mean? *Science Translational Medicine*, 341ps12.
- Huemer, M. (2009): When is parsimony a virtue?. *The Philosophical Quarterly* 59, 216–236.
- Longino, H. E. (1996): *Cognitive and Non-Cognitive Values in Science: Rethinking the Dichotomy*. Springer Netherlands, Dordrecht, pp. 39–58.
- Pusztai, L., Hatzis, C., Andre, F. (2013): Reproducibility of research and preclinical validation: problems and solutions. *Nature Reviews Clinical Oncology* 10, 720–724.

What is 'Individual Plant'?

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Plant phenome refers to the traits (or a trait), that we observe or measure, of an individual plant (its morphology, physiology, behavior). These research activities always give a lot of attention to time: the experiment designs and methods, instruments that are used, procedures that are applied are all time dependent. This is very plausible, considering that plant scientists are clearly dealing with processes not things. In this talk it will be argued: plants are processes not things and thinking life as processes is a very good way for our understanding of plant life. How plant phenome is about individual plant will be explained and, concepts of 'individual' and 'organism' will be analyzed in plant science. Pradeu (2016) says that "The broad category of biological individual can be divided into several subcategories including 'physiological individual' and 'evolutionary individual', which only partly overlap." Whether this categorization is useful and how it is, will be discussed.

The complex interaction of processes in photosynthesis activity of C3 and C4 plants in different environments will be explained as an example to point out the importance of "individual plant" in Plant Physiology Research. Phenome of an individual plant is a process that is constituted from many

complex interacting processes: evolutionary, developmental, ecological, physiological, molecular. Being a C3 or a C4 plant is a result of evolutionary processes. We may refer a distinct leaf anatomy when we say 'it is a C3' or 'it is a C4' plant. But this does not mean that we are talking about permanent features of these plants. They have different leaf anatomies and different activities in photosynthesis than each other; but all these are some kinds of stabilized activities that have been obtained and have been actively sustained through some different kinds of processes. C4 plants have evolved as having a carbon-concentrating mechanism. C4 and C3 plants have different kinds of interactions with their environments than each other. When we observe, or measure a phenotypic trait, related to photosynthetic activity, of a plant, we are aware that: the fact if it is a C3 or C4 plant affects that trait. There are many other factors that we should consider: species, sub-species, cultivar etc. of the plant (affecting its genome; so, affecting its phenotype), at which stage of development it is in, what kind of environment it is living in, what kind of environments it has lived in (affecting its phenotype and epigenome). Another very important factor is plant microbiota, which is in interaction (directly or indirectly) with all the processes of the individual plant (we may even say they are part of the plant). For example, there are many species of bacteria that is living in and around the roots of plants. They may affect plants in many ways, for example: they usually make it easy for plants to acquire nutrients from the soil (e.g. some of these bacteria, like Rhizobium, do nitrogen fixation) and they affect acclimation when plants face stress conditions.

References

- Clarke, E. (2012): Plant Individuality: A Solution to the Demographer's Dilemma. *Biology and Philosophy* 27(3): 321-361.
- Dupré, J. (2012): Processes of Life. Oxford University Press
- Dupré, J. (2018): *The Metaphysics of Evolution Article in Interface focus: a theme supplement of Journal of the Royal Society interface* · January 2018
- Leonelli, S. (2007): Growing weed, producing knowledge. An epistemic history of *Arabidopsis thaliana*. *History and Philosophy of the Life Sciences*, 29(2), 55–87.
- Pradeu, T. (2016): Organisms or biological individuals? Combining physiological and evolutionary individuality. *Biology & Philosophy*, 31(6), pp 797–817
- Taiz, L., Zeiger, E. (2010): *Plant Physiology*. Fifth Edition. Sinauer Associates, Inc.

About the limits of the chemical periodic system

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It is common to find the statement that the periodic table classifies the 92 natural elements and those synthesized by the man in the laboratory. Another important point is to take into account where the scale starts: if it does from zero or from one. The vast majority of current tables start with the element of atomic number 1, but in the last decade, Stewart (2007) has rescued speculations about the existence of an element 0. Another issue under discussion is whether the periodic table has an end. The general consensus is that when the number of protons is too large, the formation of stable nuclei will not be possible even for short periods of time (Scerri, 2013 a). There is divergence about the

position of the threshold value from which no new elements will be formed. These differences vary according to the methodology used to perform the calculation. In the calculations that assume the nucleus is a small compact point, the limit value seemed to be found in element 137 (Scerri, 2013b) according to the methodologies developed a few decades ago. Other recent calculation strategies estimate the final value at a number less than or equal to 172 (Pyykkö, 2011), or less than 173 (Indelicato and Karpov, 2013).

The debate about the possible existence of a zero element has a long history, and was re-floated in the recent years by Philip Stewart (2007), who proposed a representation of the periodic system in spiral form, known as "chemical galaxy". In the center of this representation, there is a chemical element number zero, whose "atoms" would be the neutrons. The conjecture of a "zero element" can be traced back to the works of Mendeleev, who believed in the existence of a "zero group" element, in the "zero period", that would follow the ether. Other authors who defended the conjecture were von Antropoff, Janet, Emerson, and Clark.

Recently, Zambon (2017) introduced a representation of the periodic table in which the limits are clearly defined from a system based on atomic number triads (Zambon, 2017). The triads - groups where the atomic number of the average element is equal to the arithmetic mean of the atomic numbers of the extreme elements - allow relating the elements according to their dual nature of basic substance and simple substance (Scerri, 2008, 2012). This system allows accommodating both the element 0 and the final element, using the concept of atomic number triad as a secondary classification criterion. This enables to include all the elements in a series of closed structures that are called "trees of periodicity". Together, these "trees" constitute the proposed table, in which the initial element is conceived as an undifferentiated substance that, in a certain sense, persists through all the elements, being the neutron an empirical manifestation of the element 0. This idea is in agreement with some proposals, such as Stewart's. The idea of a zero element may sound strange only if the chemical elements are considered exclusively as simple substances, characterized by their phenomenological properties. On the other hand, if their double nature as simple and basic substances is admitted, it is possible to accept the existence of a zero element, defined in a theoretical way following the same progression that orders all the elements: it would be an element that has zero electrons and zero protons, without losing its character of real entity. Also from this system, we will defend the need for the existence of a final element because, given the formal structure of the system, there must be an element of maximum atomic number. This value has the same order as that obtained by quantum-mechanical calculations and is independent of any consideration in that sense.

In the present work, we will make a summary of the different proposals formulated in order to assign limit elements to the periodic system, following a historical thread. Another point that we will defend is that the proposal of a periodic system based on triads of atomic numbers provides an alternative argument to formulate the limits of the periodic system, by using only chemical arguments such as relationships between atomic numbers, and not physical numbers based on quantum mechanics. Moreover, it also provides an argument in favor of the ontological as well as the epistemological independence of chemistry with respect to physics.

References

- Indelicato, P. , Karpov, A. (2013): Theoretical physics: Sizing up atoms. *Nature* 498: 40-41.
- Pyykkö, P. (2011): A suggested periodic table up to $Z \leq 172$, based on Dirac–Fock calculations on atoms and ions. *Physical Chemistry Chemical Physics* 13: 161-168.
- Scerri, E. (2008): The role of triads in the evolution of the periodic table: past and present. *Journal of Chemical Education* 85: 585-589
- Scerri, E. (2012): What is an element? What is the periodic table? And what does quantum mechanics contribute to the question?. *Foundations of Chemistry*, 14: 69-81.
- Scerri, E. R. (2013a): *A Tale of Seven Elements*. New York: Oxford University Press.

- Scerri, E. (2013b): *Cracks in the periodic table*. Scientific American June: 70-73.
- Stewart, P. (2007): A century on from Dimitri Mendeleev: tables and spirals, noble gases and Nobel Prizes. *Foundations of Chemistry*, 9: 235-245.
- Zambon, A. (2018): A representation of the periodic system based on atomic-number triads , *Foundations of Chemistry*, 20, 1: 51–74.

C. Philosophy of Cognitive and Behavioral Sciences

Virtual Morris Water Maze: The Independent Life of an Experimental System

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The premise of this paper is that advancements in contemporary neuroscience are best represented by developments of new experimental techniques and therapeutic innovations afforded by novel technologies. Neuroscience, similarly to other biomedical sciences, has increasingly become data-driven. These observations motivate the view developed in this paper, namely that contemporary neuroscience progresses through technological innovations as opposed to guidance of overarching theories.

This view challenges Kuhnian accounts of science such as the ones developed in Longino (2013) and Sullivan (2017). Even though the two differ in their accounts of scientific progress, both views share the Kuhnian assumptions that science is theory-driven, observation is theory-laden, and different scientific communities are bound by different theoretical, hence ontological, commitments. According to both accounts, the experimental results and their corresponding explanations produced in different scientific communities are thus incommensurable. For Longino, maintaining multiple overlapping though incommensurable approaches is, in fact, conducive to scientific progress. In her view, the challenges raised by different epistemic communities against each other, force all of them to improve and strengthen their individual knowledge producing practices. For, Sullivan, on the other hand, progress ideally consists in theoretical unification of the otherwise incommensurable approaches. She, thus, advocates unification of scientific vocabulary and standardization of experimental techniques with the goal of integration of neuroscience.

The alternative proposed here is inspired by approaches to the study of science in practice advocated by Rheinberger (1992) and Ankeny and Leonelli (2016) among others. Shifting the focus of the analysis of scientific progress from theory development to social and material practices of knowledge production, shows that while scientific communities are diverse and do not always share ontological commitments, they can nevertheless cooperate and produce integrated accounts of the phenomena of common interest. However, the progress of neuroscience cannot be reduced to the improvement of theoretical claims produced in this process. Rather, progress is measured with the generation of new hypotheses and experimental techniques articulated in this process of scientific cooperation and collaboration aided by new technological advancements.

The case for this view is illustrated by the continuous modifications of one of the most widely utilized behavioral tests in neurobiology, the Morris Water Maze. It is an experimental system initially developed as a test for rat learning and spatial memory (Morris 1981). The system has been subsequently adapted for mice and most recently for humans. In the case of humans, it is used as a virtual navigation task in neuroimaging studies. The latter development was made possible with the advancement of functional magnetic resonance imaging.

I argue that it is the opportunism afforded by the developments of new technologies that often guides the experimental process in science. In the Morris Water Maze case, the availability of neuroimaging technology enables neuroscientists to perform experiments on human subjects modeled after previously successful experiments with rodents. In this case, technology rather than some overarching theory shapes the experimental and knowledge generating practices in the field.

References

Ankeny, R. A., Leonelli, S. (2016): *Repertoires: A post-Kuhnian perspective on scientific change and collaborative research*. *Studies in History and Philosophy of Science* 60: 18-28.

Longino, H. E. (2013): *Studying Human Behavior: How Scientists Investigate Aggression and Sexuality*.

- Chicago: University of Chicago Press.
- Morris, R. G. M. (1981): Spatial Localization Does Not Require the Presence of Local Cues. *Learning and Motivation*, 12: 239-260.
- Sullivan, J. A. (2017): Coordinated pluralism as a means to facilitate integrative taxonomies of cognition. *Philosophical Explorations*, 20(2): 129-145.
- Rheinberger, H. (1992): Experiment, difference, and writing: I. Tracing protein synthesis. *Studies in History and Philosophy of Science*, 23(2): 305-331.

Interventionist Mental Causation and the Methods of Cognitive Neuroscience

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Should cognitive neuroscientists interpret the results of their studies as establishing genuine causal relations? Well, the answer depends (at least) on three factors.

1. What does 'causal relation' mean?
2. Which methods are employed in the studies?
3. How is the relation between the mental and the brain?

Woodward (2005)'s interventionist notion of causation captures, or so we argue, the notion of causation employed in cognitive neuroscience quite well. Furthermore, we coarsely distinguish between two methods: one manipulates a mental property and measures a property of the brain, the other proceeds vice versa.

The search for the neural correlates of mental functions is premised upon the idea that there is some dependency between mental properties and properties of the brain. The higher-level mental property, so maintain for example Squire et al. (2008, Ch. 53), supervenes on its lower-level brain property. For reasons of cautiousness, we assume such a dependency relation in this paper, which we call minimal supervenience. We represent properties by variables, as in the following definition.

Definition 1. Minimal Supervenience

A variable M minimally supervenes on variable P (for $M \neq P$) iff

- (i) M and P occur synchronically, and
- (ii) any change of the value of M necessarily changes the value of P.

Minimal supervenience is defined by the conditions that are common to most supervenience relations. Hence, it expresses necessary conditions for supervenience, which are depending on the philosophical view not necessarily sufficient. Nevertheless we use 'minimally supervenes' and 'supervenes' interchangeably, unless noted otherwise.

Finally, we assume that brain properties are causes of other brain properties. Given the depicted assumptions, we argue that cognitive neuroscientists should interpret the findings of their studies as establishing genuine causal relations.

In Section 2, we introduce the notion of causation assumed in cognitive neuroscience and the two methods we distinguish. In Section 3, Woodward (2005)'s interventionist account of causation for models of exclusively causal relations is presented. In Section 4, we review Baumgartner (2009)'s causal exclusion argument for the first method and Woodward (2015)'s reply. In Section 5, we find that

Baumgartner's argument does not apply with respect to the second method on Woodward's original account. We discuss the results in Section 6.

References

- Baumgartner, M. (2009): Interventionist causal exclusion and non-reductive physicalism. *International Studies in the Philosophy of Science*, 23(2):161–178.
- Carter, M., Shieh, J. (2015): *Guide to Research Techniques in Neuroscience*. Elsevier Science.
- George, M. S., Nahas, Z., Lisanby, S. H., Schlaepfer, T., Kozel, F. A., Greenberg, B. D. (2003): Transcranial magnetic stimulation. *Neurosurgery Clinics*, 14(2): 283–301, 2017/10/18.
- Gravetter, F., Forzano, L. (2011): Research Methods for the Behavioral Sciences. *Cengage Learning*,
- Kim, J. (2005): *Physicalism, Or Something Near Enough*. Princeton monographs in philosophy. Princeton University Press.
- List, C., Menzies, P. (2009): Nonreductive physicalism and the limits of the exclusion principle. *Journal of Philosophy*, 106(9):475–502.
- Martin, A., Gotts, S. J. (2005): Making the causal link: frontal cortex activity and repetition priming. *Nature Neuroscience*, 8(9):1134–1135, 09.
- Perlmutter, M. J. W., (2012): Deep brain stimulation. *Annual Review of Neuroscience*, (29):229–257.
- Romei, V., Thut, G., Mok, R. M., Schyns, P. G., Driver, J. (2012): Causal implication by rhythmic transcranial magnetic stimulation of alpha frequency in featurebased local vs. global attention. *European Journal of Neuroscience*, 35(6): 968–974.
- Sack, A. T. (2006): Transcranial magnetic stimulation, causal structure, malfunction mapping and networks of functional relevance. *Current Opinion in Neurobiology*, 16(5):593 – 599. *Neuronal and glial cell biology / New technologies*.
- Squire, L., Bloom, F., Spitzer, N., Squire, L., Berg, D., du Lac, S., and Ghosh, A. (2008): *Fundamental Neuroscience*. Fundamental Neuroscience Series. Elsevier Science.
- Windhorst, U. and Johansson, H. (1999): *Modern Techniques in Neuroscience Research*. Lab Manuals Series. Springer.
- Woodward, J. (2005): *Making Things Happen: A Theory of Causal Explanation*. Oxford Studies in the Philosophy of Science. Oxford University Press.
- Woodward, J. (2015): Interventionism and causal exclusion. *Philosophy and Phenomenological Research*, 91(2):303–347, 2015.

Phenomenology and multilevel mechanistic explanations in cognitive sciences

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In cognitive science, mechanistic explanations were proposed for such cognitive phenomena as memory (Craver 2007), visual processing (Bechtel 2009) and even consciousness (e.g. Tononi 2004, Oizumi et al. 2014). These attempts, however, often lack an adequate description of conscious experiences, and specifications of phenomena to be explained are often based on commonsensical intuitions. For example, Bechtel (2009) gives a description of neural mechanisms which are responsible for vision, however, he does not define the phenomenon using its implicit commonsensical understanding. Oizumi et al. introduce their understanding of consciousness in phenomenological axioms, which seem, however, arbitrary, and not supported by any kind of phenomenological analysis or further argumentation. Therefore, in cognitive research, it is important to introduce methodical

phenomenology, which will deliver an adequate description of the phenomenon to be explained, and will also guide the search for mechanisms.

By phenomenology, I mean a methodological way of describing conscious experience, especially the huge tradition of phenomenological philosophy started by Edmund Husserl, and developed by such philosophers as Maurice Merleau-Ponty. The objective of Husserlian phenomenology was precisely to elaborate a methodological analysis of consciousness, which was neither introspective (based on subjective inner experience) nor folk (derived from our commonsensical or linguistic intuitions). This tradition of phenomenology is cultivated today in cognitive sciences (e.g. Thompson 2007, Varela 1999). Phenomenologists and cognitive scientists also debate the possibility of naturalisation of phenomenology, that is, the possibility of integrating part of phenomenology with natural sciences. The prospect of joining these tendencies in cognitive sciences—multilevel mechanistic explanations and naturalisation of phenomenology—seems promising.

In my paper I will argue that theoretical integration of mechanistic approach in cognitive sciences and naturalised phenomenology is possible. Importantly, following Miłkowski (2016), I view theoretical integration as different from unification. First, I will show the non-reductive and multilevel character of mechanistic explanations. Then, I will argue that the weakest part of the mechanistic approach is the initial step of defining and describing the phenomenon to be explained. This step is usually associated with functional analysis, which is necessary to decompose the phenomenon into operations further associated with parts of the mechanisms. Secondly, I will argue that phenomenological analyses are in fact functional analyses, and thus can make a contribution in delivering the sketch of a mechanism. Finally, I will analyse the notion of level of explanation. I will argue that temporal distinction of levels is crucial for integrating phenomenology with mechanism. Mechanisms perform operations on differing temporal scales and frequencies. According to Craver and Darden (2013), we can use temporal distinctions of levels for a specific kind of mechanism integration, the so called inter-temporal integration, where one mechanism constitutes a temporal part of another. This method of integration seems especially interesting for phenomenology, according to which, temporality is the key aspect of consciousness. I will support this part of argument by referring to Varela (1999) who proposed an explanation of phenomenological time-consciousness in terms of neural mechanisms. Although Varela does not refer to the mechanistic approach explicitly, but uses the dynamic systems framework, his explanation can be read as mechanistic, because it describes neural mechanisms which generate specific cognitive phenomena and experiences. Furthermore, according to some researchers, a dynamic systems approach is consistent with a mechanistic one (e.g. Kaplan & Bechtel 2011).

References:

- Bechtel, W. (2009): Looking down, around, and up: Mechanistic explanation in psychology. *Philosophical Psychology*, 22:5, pp. 543-564.
- Craver, C. (2007): *Explaining the Brain. Mechanisms and the Mosaic Unity of Neuroscience*. Oxford Univ. Press.
- Craver, C., Derden, L. (2013): *In Search of Mechanisms*. The Univ. of Chicago Press.
- Kaplan, D.M, Bechtel, W. (2011): Dynamical Models: An Alternative or Complement to Mechanistic Explanations?. *Topics in Cognitive Science* 3, no. 2: 438–44.
- Miłkowski, M. (2016): Integrating cognitive (neuro)science using mechanisms. *Avant*, vol. VI no. 2. 45-67.
- Oizumi, M., Albantakis, L., Tononi, G. (2014): From the Phenomenology to the Mechanisms of Consciousness: Integrated Information Theory 3.0.. *PLOS Computational Biology* 10(5).
- Tononi, G. (2004): An information integration theory of consciousness. *BMC Neuroscience* 5:42.
- Thompson, E. (2007): *Mind in Life: Biology, Phenomenology, and the Sciences of Mind*. Harvard University Press.

Varela, F. (1999). The specious present: A neurophenomenology of time consciousness. In J. Petitot, F. Varela, J., Pachoud, B., Roy, J.-M. (eds.): *Naturalizing phenomenology: Issues in contemporary phenomenology and cognitive science* (pp. 266–329). Stanford: Stanford University Press.

Nonclassical Mereology of Odours

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1. Introduction

I argue that the mereological structure of olfactorily experienced odours, i.e. odours considered in respect of how they are perceptually experienced, is significantly different from the mereology of visually experienced objects. In particular, the mereology of olfactorily experienced odours does not satisfy the weak supplementation principle, which states that entities cannot have only one proper part. This is a significant difference, as the weak supplementation principle is an essential component of classical mereological systems.

Many philosophers of perception express a scepticism towards a “visuocentric” approach, according to which, all sensory experiences are organised by the same rules and categories as visual experiences (see Kubovy and van Vankelburg 2001). This growing suspicion has led to an increase in investigations concerning non-visual perceptual experiences, including the olfactory ones. Nevertheless, the part-structure of olfactory experiences has not been yet investigated. This is an important omission, as mereological relations play a crucial role in organising visual experiences, so showing that the mereology of olfaction is significantly different to the visual one constitutes an argument against the “visuocentric” approach to human perceptual modalities.

2. Results

According to psychological works, visually experienced objects have parts which are those of their spatial fragments that themselves are visually distinguished as objects by virtue of possessing properties that allow differentiation from other fragments of an object, or from the whole object itself. Such differentiation happens when fragments have edges separating them from the rest of an object, or at least when their edges create a point of convexity, which indicates a place where a part ends (Hoffman, Richards 1984; Xu, Singh 2002).

I argue that the same general notion of a part can be applied in case of olfactorily experienced odours and so there exists an olfactory mereology. For instance, within a complex perfume one may distinguish some components, like a cherry note and a woody note (Laska et al. 1997; Morton 2010). I claim that such components are proper parts, because they satisfy the general psychological characterisation of perceptual parts. Parts of olfactorily experienced odours are their qualitative fragments that themselves are olfactorily distinguished as odours in virtue of properties that allow them to be differentiated from other qualitative fragments of an odour, or from the whole complex odour itself.

While it is clear that visual mereology satisfies the weak supplementation principle, there are reasons to doubt whether it happens in case of human olfaction. There is a consensus that human olfactory perception has both synthetic and analytic aspects (Batty 2014; Wilson, Stevenson 2003). Typically, complex odours are not experienced as combinations of simple components constituting them, but as novel odours irreducible to their components. Nevertheless, one can often perceptually distinguish some components within a complex odour. Such results come from the studies in which participants are presented with chemical mixtures and their task is to state which simple odours can be

recognised within a complex odour (Laing, Jinks 2001; Livermore, Liang 1998). Most important in the context of these investigations, is that in some situations, participants identify only one component within a complex olfactory experienced odour.

The above result can be interpreted in three ways with differing mereological implications:

- (I) When a participant reports that the experienced odour O has only one component C , she experiences only a simple odour, identical to the component C , and not a complex odour.
- (II) When a participant reports that the experienced odour O has only one component C , in fact two components are recognised: C and the O -minus- C which is the rest of odour O without the component C .
- (III) When a participant reports that the experienced odour O has only one component C , it is literally the case that within an olfactorily experienced odour O , only one component C is distinguished, and C is not identical to the whole O . If this interpretation is the right one, then the component C is the only proper part of O , so the considered situation constitutes a counterexample to the weak supplementation principle.

I argue that it is plausible that at least some cases of distinguishing only one component are correctly characterised by the interpretation (III), and so the weak supplementation principle is false. The argumentation utilizes the fact that the proper interpretation should be consistent with the main observation about the synthetic and analytical character of human olfaction, i.e., that complex olfactorily experienced odours have a structure containing simple components, but they are something more than just a combination of these components.

3. Conclusion

The human visual and olfactory experiences seem to be mereologically disunified. It is so because the olfactory part-structure does not satisfy the weak supplementation principle, as there are olfactorily experienced odours which have only one proper part. Hence, the olfactory mereology is significantly different from the visual mereology in which the weak supplementation principle is satisfied.

References

- Batty, C. (2014): Olfactory Objects. In: Biggs, S., Stokes, D., Matthen, M. (eds.): *Perception and its Modalities*. New York: Oxford University Press. 222-224.
- Hoffman, D. D., Richards, W. A. (1984): Parts of Recognition. *Cognition*, 18(1-3), 65-96.
- Kubovy, M., von Valkenburg, D. (2001): Auditory and Visual Objects. *Cognition*, 80, 97-126.
- Laing, D. G., Jinks, A. L. (2001): Psychophysical Analysis of Complex Odour Mixtures. *Chimia*, 55, 413-420.
- Laska, M., Distel, H., Hudson, R. (1997): Trigeminal Perception of Odorant Quality in Congenitally Anosmic Subjects. *Chemical Senses*, 22(4), 447-456.
- Livermore, A., Laing, D. G. (1998): The Influence of Chemical Complexity on the Perception of Multicomponent Odor Mixtures. *Perception & Psychophysics*, 60(4), 650-661.
- Morton, T. H. (2000): Archiving Odors. In: Rosenfeld, S. (ed.): *Of Minds and Molecules*. New York: Oxford University Press. 251-272.
- Wilson, D. A., Stevenson, R. J. (2003): Olfactory Perceptual Learning: The Critical Role of Memory in Odor Discrimination. *Neuroscience and Biobehavioral Reviews*, 27, 307-328.
- Xu , Y., Singh, M. (2002): Early Computation of Part Structure: Evidence From Visual Search. *Perception and Psychophysics*, 64(7), 1039-1054.

D. Philosophy of Social Sciences

Memetics as pseudoscience

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Since Richard Dawkins has coined the term meme in 1976 (Dawkins, 1976/1989), many scholars became working on the new paradigmatic discipline of memetics. The theory is built on analogy with biological neo-darwinian evolutionary theory. While there is a gene considered as the main unit of replication and therefore selection in biology, meme takes this function in evolution of culture. Memeticists are taking the meme's eye view on the evolution which simply means that memes are selfish in the same way genes are. They do not spread due to they pose benefits on their bearers, rather because they are replicators which replicate for their own sake. Memes compete with each other for space in the memory and therefore also for human attention (e.g. Blackmore, 1999 or Dennett, 1995). They are transmitted by social learning (imitation according the majority of authors) and errors in that learning are generally understood as analogies to genetic mutations (Blackmore, 1999). Those mutants, which are successful in getting and keeping attention, are transmitted and could participate on cultural evolution. Memetics seems like a real science transmitted to the social science from biology, some memetic researchers speak about a new paradigm (Brodie, 2009; Gatherer, 1997) that could solve all the big questions such as: how the big brains, language (Blackmore, 1999) or religions (Dawkins, 1996; 2006) evolved or what consciousness is (Dennett, 1991).

Although there were big expectations from the memetic science, there are still no results. Some authors think that it is because the science itself is just in the phase of defining theoretical grounds (Aunger, 2000; 2001; Hull, 2001). Good argument for this claim could be that memetics began to be in the main interest in the 90s (Burman, 2012; Hull, 2001). The majority of debates about memetics were also focused on the precise definition of memes and about their ontology. This question gave rise to two groups of memeticists. There are on the one hand those who put memes into the brain or memory and think about physical objects as about meme's phenotypes (Lynch, 1991; Delius, 1991; Jan, 2007; Dawkins, 1991) and on the other hand those who put them outside the brain into physical objects (Benzon, 1996; Sterelny, 2006; Gatherer, 1998). However, the fact still remains, that memetics was not only able to enrich the current knowledge about how culture evolves (Edmonds, 2005; Kuper, 2000) but that it is not even in principle able to do that. The reason is that memetics could not generate any falsified predictions which are not reducible into language of psychology and cognitive science (Coyne, 1999). This presentation will show that this constraint is present in all possible ontologies of memes. It is due to the way memes are selected. There are human brains that select which memes will spread. Hence anytime we ask why this particular meme is transmitted more than others, we have to ask which brain structures does allow it. The concept of meme is not necessary in the answering that kind of questions. Memetics is therefore only pseudoscience that could think about itself as about very abstract theoretical framework for making explanations more easy to understand.

Why then memetics did become so popular among many scholars and why prominent authors do not want to think about it in strictly scientific way although they present it in that way (Dennett, 1995; 2017)? This question needs to look at the way memetics itself is transmitted, which kind of medium it uses to spread (Beneš, 2004) and also what purpose does it fulfill in the broader context of ideas and argumentation of such authors as Daniel Dennett, Richard Dawkins or Richard Brodie. Memetics is mainly published in popular books (Brodie, 2009; Dawkins, 1976; Distin, 2005; Aunger, 2001) and not in scientific journals. Richard Dawkins uses memetics in his anti-theistic argumentation (Dawkins, 2006) and Richard Brodie even tries to show people how to change their lives (Brodie, 2009). Memetics seems to be a good device for both of them. This is due to two reasons. First is that memetics is complete theory. It has explanation for the culture as the whole unit. The second is that

those explanations are not completely dependent on biology and so it allows humans to get rid of selfish genes (Dawkins, 1989).

References

- Beneš, K. (2004): S. Blackmorová: Teorie memů - co je to za literaturu?. In: Nosek, J. (ed.): *Memy ve vědě a filosofii?: sborník příspěvků* (pp. 73-75). Praha: Filosofia - nakladatelství Filosofického ústavu AV ČR.
- Benzon, W. (1996): Culture as an evolutionary arena. *Journal of Social and Evolutionary Systems*, 19(4), 321-362. doi:10.1016/s1061-7361(96)90003-x
- Blackmore, S. J. (1999): *The meme machine*. Oxford: University Press.
- Brodie, R. (2009): *Virus of the mind: the new science of the meme*. Carlsbad, CA: Hay House.
- Burman, J. T. (2012): The misunderstanding of memes: Biography of an unscientific object, 1976–1999. *Perspectives on Science*, 20(1), 75-104. doi:10.1162/posc_a_00057
- Coyne, J. A. (1999): The self-centred meme. *Nature*, 398(6730), 767-768. doi:10.1038/19677
- Dawkins, R. (1989): *The selfish gene* (New ed.): Oxford: Oxford University Press.
- Dawkins, R. (1996): Viruses of the Mind. In: Dahlbo, B (ed.) *Dennett and His Critics: Demystifying Mind*. Oxford: Blackwell.
- Dawkins, R. (2006): *The God Delusion*. London: Bantam Press.
- Delius, D. J. (1991): The nature of culture. In Dawkins, M. S., Halliday, T., Dawkins, R. (eds.): *The Tinbergen legacy* (pp. 75-99): London: Chapman & Hall.
- Dennett, D. (1995): *Darwin's Dangerous Idea: Evolution and the Meanings of Life*. London: Penguin Books.
- Dennett, D. C. (1991): *Consciousness explained*. Boston: Little, Brown and Company.
- Dennett, D. C. (2017): *From bacteria to Bach and back: the evolution of minds*. New York: W. W. Norton & Company.
- Distin, K. (2005): *The Selfish Meme*. Cambridge: Cambridge University Press.
- Edmonds, B. (2005): The revealed poverty of the gene-meme analogy – why memetics per se has failed to produce substantive results. *Journal of Memetics - Evolutionary Models of Information Transmission*, 9. http://cfpm.org/jom-emit/2005/vol9/edmonds_b.html
- Gatherer, D. (1997): Macromemetics: Towards a Framework for the Re-unification of Philosophy. *Journal of memetics – Evolutionary Models of Information Transmission*, 1. http://cfpm.org/jom-emi/1997/vol1/gatherer_dg.html
- Gatherer, D. (1998): Why the Thought Contagion Metaphor is Retarding the Progress of Memetics. *Journal of Memetics - Evolutionary Models of Information Transmission*, 2. http://cfpm.org/jom-emit/1998/vol2/gatherer_d.html
- Hull, D. L. (2000): Taking Memetics Seriously: Memetics Will be What we Make it. In: Aunger, R. (ed.): *Darwinizing Culture: the Status of Memetics as a Science* (pp. 43-67): Oxford: Oxford University Press.
- Jan, S. B. (2007): *The memetics of music: a neo-Darwinian view of musical structure and culture*. Aldershot: Ashgate.
- Kuper, A. (2000): If Memes Are the Answer, What Is the Question? In: Aunger, R. (ed.): *Darwinizing Culture: the Status of Memetics as a Science* (pp. 175-188): Oxford: Oxford University Press.
- Lynch, A. (1991): Thought contagion as abstract evolution. *Journal of Ideas*, 2(1), 3-10.
- Sterelny, K. (2006): Memes Revisited. *The British Journal for the Philosophy of Science*, 57(1), 145-165. doi:10.1093/bjps/axi157

Valuing non-market goods for intergenerational investments – explicit moral judgements in willingness to pay and willingness to accept compensation

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Intergenerational human activities, like investments protecting climate or biodiversity, influence the welfare of unborn, future generation. In public policy-making, the cost-benefit analysis (CBA) is used to measure efficiency of such activities via welfare changes. CBA is particularly useful here, as multiple impacts are non-market and are valued on the basis of willingness to pay (Zerbe & Bellas 2007, Spash & Hanley 1994, Pearce et al. 2006). Additionally, CBA uses valuation done by present generation which is the only feasible option since the preferences of future unborn people are not available at the moment of making the decision. This inevitably interlinks the question of efficiency and ethical judgement, firstly because the present generation makes choices on behalf of people unable to participate in contemporary decisions, secondly, due to the fact that investing to benefit future people involves outlays borne today (Page 2007, Gardiner 2006).

However, a discrepancy can be observed between CBA theoretical foundations and practice. Canonical CBA rests on utilitarian basis, which apply consequentialist framework that limits the scope of the analysis as deontological values cannot be properly reflected (Zerbe 2016). At the same time, analysts implicitly make moral decisions, starting from deciding which impacts are of economic relevance or which method of non-market good valuation should be applied or what discount rate is applied. Last but not least, valuation of non-market goods done by contemporary people that bases on their willingness to pay (WTP) or willingness to accept compensation (WTA) may include ethical judgements as well, observed via non-use values or protest bids in contingent valuation questionnaires (Meyerhoff & Liebe 2006, Spash et al. 2009, Diafas et al. 2017). This could be justified by consumer-citizen mixed framework and distinction between preferences and attitudes (cf. Sagoff 1986, Orr 2007).

The purpose of the paper is then to incorporate some elements of deontology into consequentialist basis of CBA to limit the inconsistency between theory and practice. I argue that this could be done by incorporating explicitly deontological values in WTP and WTA for valuing non-market goods in evaluation of intergenerational investment.

I propose to measure utility of an individual as consisting of two parts: a function of utility derived of income (y), the good affected by the intergenerational investment (G) and moral satisfaction due to warm glow (M), and the second part which is existence value (E) attributed to the good. $U=u(y,G,M)+E$

On this basis I propose a definition of WTP and WTA that would incorporate consequentialist framework via the first part, on the assumption that it reflects the value people place on changes in welfare itself (for themselves or due to altruism, to future people or other species), and the second part, which would reflect their deontological preferences, values not related to welfare, but the existence of the good (a deontological extension of utilitarianistic framework is developed also in regard to animal welfare, cf. Johansson-Stenman 2018).

The formulation allows to distinct 3 elements in WTP or WTA. The first (ΔG) relate to changes in the good itself and how it will affect the use values provided by it, the second (ΔM) reflects individual satisfaction derived from increasing the welfare of others, and finally the third one, (ΔE), which mirror additional value placed on the change in good due to moral values. I argue that this modification goes some way in overcoming the flaws of consequentialism in CBA.

The separation might be relevant for practical applications for intergenerational investments as it may capture the values placed on very remote future, which cannot be explained by self-centred

utility changes, i.e. parental altruism, or in cases of investments that burden mainly the present generation with high uncertainty over future impacts, as a monetary reflection of precautionary principle. Additionally, analysts as well as people declaring their bids, in a setting where moral values are investigated explicitly, may focus more on their citizenship instead of purely consumer perspective.

However, it must be highlighted that the tools for eliciting separately those elements, still need development. Contingent valuation could be probably the most successful in this respect due to its elasticity in constructing the questionnaire, however is not free from biases. Alternatively, deliberative monetary valuation may be used as it may focus in more detailed way on advantages and disadvantages of proposed actions.

References

- Diafas, I., Barkmann, J., Mburu, J. (2017): Measurement of Bequest Value Using a Non-monetary Payment in a Choice Experiment—The Case of Improving Forest Ecosystem Services for the Benefit of Local Communities in Rural Kenya. *Ecological Economics*, 140, 157-165.
- Johansson-Stenman, O. (2018): Animal Welfare and Social Decisions: Is It Time to Take Bentham Seriously?. *Ecological Economics*, 145, 90-103.
- Gardiner, S. M. (2006): A perfect moral storm: Climate change, intergenerational ethics and the problem of moral corruption. *Environmental values*, 397-413
- Page, E. A. (2007): *Climate change, justice and future generations*. Edward Elgar Publishing.
- Meyerhoff, J., Liebe, U. (2006): Protest beliefs in contingent valuation: explaining their motivation. *Ecological economics*, 57(4), 583-594.
- Orr, S. W. (2007): Values, preferences, and the citizen-consumer distinction in cost-benefit analysis. *politics, philosophy & economics*, 6(1), 107-130.
- Pearce, D., Atkinson, G., Mourato, S. (2006): Cost-benefit analysis and the environment: recent developments. *Organisation for Economic Co-operation and development*.
- Sagoff, M. (1986): Values and preferences. *Ethics*, 96(2), 301-316.
- Spash C. L., Hanley N., (1994): *Cost-Benefit Analysis and the Environment*. Edward Elgar, Aldershot
- Spash, C. L., Urama, K., Burton, R., Kenyon, W., Shannon, P., Hill, G. (2009): Motives behind willingness to pay for improving biodiversity in a water ecosystem: Economics, ethics and social psychology. *Ecological Economics*, 68(4), 955-964.
- Zerbe, R. O., Bellas, A. S. (2007): *A primer for cost-benefit analysis*. Northampton, MA: Edward Elgar
- Zerbe R. (2016): A Distinction between Benefit-Cost Analysis and Cost Benefit Analysis Moral Reasoning and a Justification for Benefit Cost Analysis.
<https://evans.uw.edu/sites/default/files/A%20Distinction%20Between%20BCA%20and%20CBA. March%2C%20WITHOUT%20ID%2C%202016.pdf>

The normative theory of decision making in economics. A philosophical evaluation

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The normative understanding of a theory is accepted in economics, decision theory, as well as in philosophy of economics and in the philosophical analysis of theories of decision-making (see e.g.: Hausman and McPherson 2006, Peterson 2009, Baron 2004, Briggs 2014, Bell and Raiffa 1988). The common view is that the neoclassical economics consists (at least in part) of normative theories, whereas the behavioural economics is descriptive and aims at explanation of how agents in economic settings ‘really behave’. The normative status of theories of individual decision making in neoclassical

economics is often linked to their affinities with the so-called rational choice theory. It is claimed that these theories specify how rational agents should behave.

It has been observed (see: Hands 2015) that even though the normative view on decision theory has become widely influential among mainstream economists, it was not always there. It is argued that before the so-called “normative turn”, (neoclassical) economists didn’t treat their theories and models (especially – the expected utility theory, EUT) as normative, but only as descriptive ones (Herfeld 2016). EUT was supposed to explain how people make choices and behave, and not to indicate how they ought to behave. Why did the turn take place?

Hands reports that it is often believed that it might have been a defensive methodological move of the neoclassical economists who had tried to defend their research programme, despite the large experimental and empirical evidence that falsified it. Heukelom (2014) mentions that the distinction between a descriptive and normative theory arrived to economics from experimental psychology – mainly due to the success and influence that Tversky and Kahneman had on the development of behavioural economics. At the same time Heukelom points out that thanks to the dual interpretation of EUT the behavioural economics entered the mainstream economic research.

But can the claim that EUT serves this dual function – of explaining behaviour and of indicating the norms of rational behaviour – be supported and justified? Is the distinction between descriptive and normative theory defensible methodologically? Can it be defended from the philosophy of science point of view? The aim of the paper is to address these questions, as well as to analyse what are the consequences of accepting the dual function of EUT – for economic theorizing, as well as for the policy implication, especially those that are derived today from the behavioural economics.

The structure of the paper is as follows. First, I review the diverse ways in which the views on the normative and descriptive interpretation of decision theory have been formulated and defended by the leading scholars in the field and I attempt to explicate them. Second, I briefly sketch the historical developments in the studies on the individual decision-making. Third, I question the reasons given in the philosophical literature for why the decision theory could be interpreted normatively. Fourth, I indicate the methodological problems that the acceptance of the distinction leads to. Fifth, I conclude.

The ‘why’ and ‘how’ of causal inferences in economics

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Economists are known to draw unjustified causal conclusions. Recently, the widely-discussed and criticized correlational analysis (Reinhart and Rogoff 2010) was employed to justifying macroeconomic policy-making (Maziarz 2017a). Pol (2013) indicated that economists employ several different methods to justify causal claims. Maziarz (2017b) indicated the lack of research on empirical adequacy of the philosophical theories of causality to the research practice of economists and implicit understanding of this metaphysical concept). Considering that economists are rarely explicit about their philosophical presuppositions regarding causality (Claveau and Mireles Flores 2014), to fulfill this gap, main theories of causality must first be operationalized in the context of economics to highlight philosophical presuppositions of methods of causal inferences employed by economists.

There are six main theoretical approaches (families of theories) to causality in philosophy that are relevant for the context of economics. My purpose is to operationalize these approaches to (understanding of) causality with the aim at addressing the question which of the methods employed by economists to drawing causal conclusions are justified and, precisely, which of the six theories

justifies each method. For instance, the Granger-causality tests (Granger 1969; 1980) employ the definition of causality promoted by the probabilistic account (e.g. Sims 1972), cf. Maziarz (2015). Additionally, the project of theoretical macroeconomics (vector autoregression models) is also grounded in this approach (cf. Cooley and LeRoy 1985).

In contrary to these recent developments in the theoretical econometrics, the usual Cowles Commission approach is grounded in the regularity approach to causality that dates back to the Humean stance. The Cowles Commission approach was developed by Simon (1977) who employed the manipulationist approach to causality and argued in favor of using interventions to solve the indeterminacy of causal direction by data. Theoretical causal modeling draws from the mechanistic approach to causality. The main conclusions of the presentation are as follows. First, in the contemporary economics, there are several approaches to causal inferences that differ regarding both methods and philosophical underpinnings. Second, economists seem not to be aware of the philosophical grounds of the methods they use. Third, the dominance of the data-driven approach indicates that the criticism voiced by economic methodologists in the aftermath of the 2007-2008 financial crisis was taken to hearts by economists.

References

- Claveau, F., Mireles-Flores, L. (2014): On the meaning of causal generalisations in policy-oriented economic research. *International Studies in the Philosophy of Science*, 28(4), 397-416.
- Granger, C. W. J. (1980): Testing for causality. A personal viewpoint. *Journal of Economic Dynamic and Control*, 2(4), 329-352.
- Granger, C. W. J. (1969): Investigating causal relations by econometric models and cross-spectral methods. *Econometrica*, 37(3), 424-438.
- Maziarz, M. (2015): A review of the Granger-causality fallacy. *The journal of philosophical economics: Reflections on economic and social issues*, 8(2), 86-105.
- Maziarz, M. (2017a): The Reinhart-Rogoff controversy as an instance of the ‘emerging contrary result’phenomenon. *Journal of Economic Methodology*, 24(3), 213-225.
- Maziarz, M. (2017b): Causation in Economics. The Most Recent Analyses and the Unsolved Problems. *Annales. Ethics in Economic Life*, 20(1):
- Reinhart, C. M., & Rogoff, K. S. (2011): From financial crash to debt crisis. *The American Economic Review*, 101(5), 1676-1706.
- Sims, Ch. (1972): Money, income, and causality. *The American Economic Review*. 62(4), 540-552.
- Woodward, J. (2002): What is a mechanism? A counterfactual account. *Philosophy of Science*, 69(S3), 366-S377.
- Cooley, T. F., & LeRoy, S. F. (1985): Atheoretical macroeconomics: a critique. *Journal of Monetary Economics*, 16(3), 283-308.
- Simon, H. (1977): Causal ordering and identifiability. In Simon, H. (ed.): *Models of Discovery* (pp. 53-80): Dordrecht: Springer, 55-80.

Thinking through Kinds: the Ontological Turn meets Interpretive Social Science

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There is an ever-increasing literature and ever-intensifying debate on the so-called ‘ontological turn’ in anthropological methodology. Although most accounts of the turn’s theoretical underpinnings (see, for instance, the recent monograph-length treatment of its main theses by Martin Holbraad and

Morten Pedersen) are quick to accentuate how the turn in question is strictly methodological, the proposed talk wishes to argue otherwise. The primary aim of the talk is to show how the insight gained from the fieldwork carried out under the aegis of the 'ontological turn' could be readily incorporated into a more encompassing theory of interpretive social science – one that is focused on understanding social phenomena as members of conceptually constituted (and rule-governed) social kinds.

The talk attempts to draw simultaneously on the two most influential theories of interpretive social science that shaped social theoretical thinking in the 20th century (those of Max Weber and Peter Winch) and the work of Ian Hacking considering the constitution and maintenance of social or human kinds. The outlines of the argument are the following: in order to gain insight about social phenomena, one has to understand and interpret what one wishes to know about. Taking interpretive sociology as a paradigmatic example, this could be done by either understanding subjectively intended meanings (the Weberian way) or the communally shared rules that equip social practices with their meaningfulness (the Winchian way). The gap between the two approaches (one seemingly individualistic and the other collectivistic in its outlook) might, however, be bridged by taking a different stance towards social processes and phenomena, that is viewing them as social kinds that are intersubjectively real and conceptually constructed (or, to paraphrase an analogy from John Searle, epistemologically real and ontologically constructed). Showing the implications of such a model of interpretive social science, the talk wishes to place the theoretical aspects of the 'ontological turn' within its boundaries, and draw attention to how ontological anthropology might help realizing the aims of a social science that takes scientific understanding to be primarily the interpretation of social kinds and the rules that play a crucial part in their constitution. Formulating the point a fair bit stronger: ontological anthropology is already practicing that kind of interpretation without reflecting on it – and that absence of reflection could prove to be detrimental to its philosophical aptness and adequacy.

The final part of the talk addresses the charges that might be levelled against ontological anthropology (and that might disqualify it from the realm of viable alternatives of current social scientific methodologies) and attempts to partially modify the framework in order to answer the objections (the claim that this novel methodological approach is proposing an untenable meta-ontology that is never made explicit in its formulations; or the claim that the radical relativism seemingly hardcoded into the ontological turn's foundations prevents it from being a philosophically sound approach to the interpretation of social phenomena). After addressing these objections, it will hopefully be shown how a novel way of doing anthropological fieldwork might inform a novel way of social scientific interpretation.

E. History, Philosophy, and Social Studies of Science

Re-integrating HPS: Scientonomy as a Missing Link

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From a philosophical perspective, one major rationale for an integrated HPS was the idea that the historical record of transitions in sciences could be used to test general philosophical claims about science. Once established, these general philosophical claims would then provide a theoretical foundation for explaining individual historical transitions in sciences (Lakatos 1971, Donovan et al. 1992). It is safe to say that this bold project of an integrated HPS has never really come to fruition (Caneva 2012). While philosophers generally agree that an historically-informed philosophy of science is a worthy undertaking, most professional historians refuse to see its value. Despite a growing number of attempts to re-integrate HPS (Schickore 2011), there is still a considerable institutional, disciplinary, and methodological gap between mainstream history and mainstream philosophy of science. There is currently no clear consensus on what an ideal integrated HPS should look like and, specifically, on how this integrated HPS might address the historical reasons that led to the exodus of historians. In this paper, we attempt to identify the key reason of the dis-integration of the original HPS and outline a new approach that can fruitfully re-integrate key components of both history and philosophy of science.

We maintain that the dis-integration of the original HPS was mostly due to the conflation of two distinct projects: the search for a *descriptive* general theory of scientific change and the search for a *normative* methodology of science. Attempts to test or even merely illustrate *normative* methodological dicta by means of historical case studies have been rightfully scorned by historians (Williams 1975, Shapin 1982) and questioned by some philosophers (Pitt 2001, Laudan & Laudan 2016). These attempts would bluntly ignore the fact that the methods of theory evaluation are not fixed, but change through time; they would ignore that the actual methods employed in theory evaluation could differ drastically between different epistemic communities, different fields of inquiry, and different time periods. Consequently, they would often result in *shoehorning* ill-documented historical cases into the confines of a chosen normative methodology (e.g. Lakatos & Zahar 1976). Since the search for a *normative* methodology of science was not separated from the search for a general *descriptive* theory of scientific change, these unfortunate misconstructions of historical cases eventually convinced mainstream historians that any general claims about science – descriptive or normative – are doomed to distort our historical narratives and inevitably produce a caricature of a history. As a result, the contemporary history of science has taken an explicitly *a-theoretical* stance and revels in its insistence on the apparent disunity of historical cases.

We argue that in order to successfully re-integrate HPS, we need to appreciate that there is a missing link between the *descriptive* history of science and the *normative* philosophy of science; this missing link, we contend, is the *descriptive general theory of scientific change*. Thus, instead of continuing the questionable practice of illustrating normative philosophical claims by means of cherry-picked historical case studies, the philosophy of science, we suggest, must rely on the findings of a general descriptive theory of scientific change. Similarly, what any good historical narrative needs as its

backbone is not some *normative* dicta of this or that methodology *a la* Lakatos or Laudan, but an accepted *descriptive* theory that uncovers the general patterns of changes both in theories and in methods of their evaluation. As evidence for this proposal, we will consider the work currently being done by a community of scholars that aim at establishing an empirical descriptive science of science named *scientonomy* (www.scientowiki.com). Our goal is not only to outline how scientonomy can potentially bridge the gap between history and philosophy of science, but to show precisely how this re-integration has already been implemented by the scientonomy community. By considering the theoretical underpinnings of scientonomy (Barseghyan 2015), we will demonstrate how it addresses the historians' concerns that led to the dis-integration of HPS, and how it offers a fruitful way towards a re-integrated HPS.

References

- Barseghyan, H. (2015): *The Laws of Scientific Change*. Springer.
- Caneva. K. (2012): What in Truth Divides Historians and Philosophers of Science? In: Mauskopf, S., Schmaltz, T. (eds.) (2012): *Integrating History and Philosophy of Science*. Springer, pp. 49-56.
- Donovan, A., Laudan, L., & Laudan, R. (eds.): (1992): *Scrutinizing Science. Empirical Studies of Scientific Change*. The Johns Hopkins University Press.
- Lakatos I. (1971): History of Science and its Rational Reconstructions. In: Buck, R.C., Cohen, R.S. (eds.): (1970): *PSA 1970. Boston Studies in the Philosophy of Science, vol 8*. Springer.
- Lakatos, I., Zahar, E. (1976): Why did Copernicus's Research Programme Supersede Ptolemy's? In: Lakatos, I. (1978): *Philosophical Papers, volume I*. Cambridge University Press, pp. 168-192.
- Laudan, L.,Laudan, R. (2016): The Re-emergence of Hyphenated History-and-Philosophy-of-Science and the Testing of Theories of Scientific Change. *Studies in History and Philosophy of Science* 59, pp. 74-77.
- Pitt, J.C. (2001): The Dilemma of Case Studies: Toward a Heraclitian Philosophy of Science. *Perspectives on Science* 9 (4), pp. 373-382.
- Schickore, J. (2011): More Thoughts on HPS: Another 20 Years Later. *Perspectives on Science* 19 (4), pp. 453-481.
- Shapin, S. (1982): History of Science and its Sociological Reconstructions. *History of Science* 20, pp. 157-211.
- Williams, L.P. (1975): Review: Should Philosophers Be Allowed to Write History? *The British Journal for the Philosophy of Science* 26 (3), pp. 241-253.

From Mathematical to Physical Coordination and Back. Why Mathematical Coordination Can Be More Entangled than it Looks Like

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Over the past decade, especially in the literature about measurement, there has been a revival of the notion of "coordination", which has been given great impulse following van Fraassen's *Scientific Representation* (2008). The "problem of coordination", as van Fraassen refers to it, was originally presented by Reichenbach in his early work, and it involves understanding how to coordinate abstract,

empty structures with “pieces of reality”. No matter what formal (mathematical) system of equations we may employ to represent physical events, it will be lacking the very fundamental statement determining its *validity* for (i.e., applicability to) reality. In Reichenbach’s 1920 account, coordination is classically defined as a form of mapping. However, there is a difference between mathematical and physical coordination in that physical objects cannot be merely determined by virtue of axioms and definitions as it seems to be the case of mathematical objects. In the case of physical objects, in fact, the formal component determines the “individual things of the undefined side” but the order of the defined side is “prescribed” by the undefined side. The coordination to “undefined” elements is restricted, not arbitrary, and this restriction, as Reichenbach emphasizes, is “the determination of knowledge by experience” (1920: 42).

Our mathematical representation of reality rests on principles and equations that are expressed in mathematical form but that involve terms that supply a connection to the real world. In van Fraassen’s view, solving a coordination problem typically implies narrowing down a variety of logical possibilities to a particular structure or class of structures that provide scientific representations of a target system. Seen from within the historical process in which theoretical parameters become coordinated with their correlate, the theory appears to evolve together with measurement procedures in an entangled way. In van Fraassen’s terminology, we should look at this process using a synoptic vision, combining the description “from above” (that considers parameters as having already reached a certain stability and being theoretically embedded in a mature theory) with the description “from within” (that considers the historical process in which measurement procedures that led to the definition of those parameters were developed besides and within the theory that uses them (2008: 124-136).

However, there are cases where such evolution reveals a deeper level of entanglement in the very formation of theoretical terms and delimitation of the theory’s domain also when it comes to mathematical objects. For instance, in Dedekind’s definition of irrational numbers, the idea of a correlation between known and unknown terms *precedes* their embedding into a larger structure. As observed by Dedekind, the system of rational numbers can be divided into two disjoint classes so that each number of the first class precedes each number of the second, and one number produces the division as the upper limit of the first class (or the lower limit of the second). Dedekind defined the set of real numbers by introducing irrational numbers as symbols for those divisions that do not correspond to any rational number in the original domain.

In the projective model of non-Euclidean geometry, there is a shift from the study of particular relations of congruence to the formation of generalized spatial concepts. The possibility of different geometries posed the problem of clarifying the notion of congruence according to the operations that are admissible under different geometrical hypotheses. The properties of space were identified as the necessary and sufficient condition for carrying out such operations.

According to van Fraassen, the starting point in the work of mathematicians such as Bernhard Riemann and Felix Klein was the view that all of the infinitely many geometries are equally possible from a logical point of view and on a par as mathematical possibilities. Van Fraassen identifies Klein’s projective model as an “initial locus for the problem of coordination” insofar as Klein imposed different congruence relations, actually corresponding to different measurement standards, on the same space of projective geometry. On this account, metrical projective geometry is characterized as a particular class of geometries, in which congruence allows, in practice, for different interpretations. However, the development of Klein’s thought shows that, on the contrary, metrical projective geometry foreshadowed the idea of a general classification of geometries. Klein’s projective model of 1871 offered the first concrete example of a group-theoretical classification as sketched by Klein in the “Erlanger Programm” (1872). The implementation of the programme presupposed further mathematical achievements, including a comprehensive theory of continuous transformation groups as articulated only later by Sophus Lie (1888–1893). In Klein’s view, metrical projective geometry

provided, nonetheless, a clarification of concepts as the first geometrical interpretation of the algebraic theory of invariants. In other words, Klein gained insight into the abstract notions of group, invariance, and symmetry from within their geometrical application in a time when the theory of groups as we know it (i.e., the view from above) was still in the making.

The aim of this paper is to discuss the originality of the type of coordination that can be involved in these mathematical procedures and thus to clarify the extent to which the process leading to the formation of theoretical concepts within mathematical coordination can be more entangled than it has so far been interpreted. This leads to a new view of the difference between mathematical and physical coordination, as we will argue.

When science becomes a problem. Identity, biology and politics

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The classical problem of personal identity is widely discussed in analytic philosophy, exclusively as a metaphysical-theoretical issue. That is striking, given that the answers the question of identity has received –since its first formulation, with John Locke (1975 [1694])– appeal to notions that nowadays are defined by science, rather than by philosophy.

Locke had proposed that *memory* is the foundation of personal identity, initiating the psychological view (Noonan 2003, Parfit 1971, among others). According to this view, the continuity of consciousness (*memory*) is what makes everyone who they are, and distinguishes each person from everyone else. This notion has been challenged by the physical view: the idea that we are no different from *biological organisms* or *bodies*, hence identity rests on the continuity of an organism, according to animalism (Mackie 1999, Olson 1997), or on the continuity of a *human body* (Williams 1956-7, Thomson 1997). When we ask what memory is or what a biological organism is, philosophy lags behind science.

At present, a genetic approach is presented in several debates as an adequate candidate to define personal identity, and can be considered a scientific successor of the physical conception because it places identity on the genetic material of an organism. Given this state of affairs, it seems reasonable to expect philosophy of science to engage in the debate, but regrettably so far it has not. Furthermore, some social practices and political demands make this a far more complex picture. Science is appealed in several political strategies for the broadening of rights concerning identity, as having an emancipatory and progressive role.

We will discuss some current political practices and claims in Argentina that situate identity and genetics in the foreground. We will analyze the demand of *identity-restitution* of the appropriated sons and daughters of the *detained-disappeared* people during the last Argentinian's Military Dictatorship (1976-1983). Approximately 500 children were appropriated –robbed from their biological parents and raised unaware of their origins by other families, generally related to members of the military force. The association *Abuelas* (Grandmothers) de Plaza de Mayo (APM) was founded in 1977 with the demand for finding the children –currently adults– and *restituting their identities*. The work of APM led to the development of the “grandparenthood index” (the scientific measurement of the probability of a biological link between children and grandparents in absence of the parents) and to the incorporation

of some articles in the *United Nations Convention on the Rights of the Child*, establishing the “right to identity”.

And –related to but not coextensive with the restitution demand– we will analyze the comprehension of the “right to identity” as the right to know the biological filiation, defended by Argentinian associations of (legally and illegally) adopted people –such as *Raíz Natal* (Birth Roots)– and some associations devoted to the defense of the rights of children born by assisted reproduction techniques. The *right to identity* –to “genetic identity”– is the key in the claims of these associations, since it is deemed that every person has the right to know their biological origin (Gesteira 2014).

Nevertheless, genetics has usually been considered regressive, since it can only provide a conservative, based on blood-ties conception of identity (Gatti 2014). What is more, identity based on pure biological facts is the identity of a *thing* rather than *personal* identity (Agamben 2011). Besides, the idea that genetics defines us is generally upheld by conservative political positions. In fact, Argentina’s dictatorship separated the detained-disappeared persons from their sons and daughters, under the assumption that their *inherited identity* had to be erased and re-written. Several testimonies show that the appropriators finally found that unsuccessful, since *genetics is stronger than nurturing*.

If appropriation tried to nullify children’s identities –to “save” them from their parents (Villalta 2012)–, then restitution must (and can) correct it, since, in a certain way, also from the restitution perspective, *genetics is stronger than nurturing*. So, science, by means of the “truth” revealed by the DNA tests, allows the purpose of restitution –a demand for justice and a political strategy of resistance– to be fulfilled, while it is usually assumed that to politicize a phenomenon is to de-*biologicize* it. Thus, genetics becomes a problem that must be addressed. One common position states that a genetic approach is a powerful strategy as it aids the resolution of crimes and the achievement of justice, but should not be regarded as *defining* identity or used in other areas. We find this view misguided.

The theoretical debate and the analysis of the cases show the necessity of asking if the metaphysical problem of identity can legitimately be “solved” by science, and which is the role of science as bio-power. We hope this analysis will show that these are not two different problems, but one: biotechnology operates over people’s lives, to a large extent, because science is thought as defining, among other things, *what* and *who* we are. Identity reveals itself as *managed* and *administrated* by science.

References

- Agamben, G. (2011): *Identidad sin persona*. *Desnudez*, Buenos Aires: Adriana Hidalgo.
- Gatti, G. (2014): Las Abuelas, el gobierno de la sangre y la banalidad del bien . *Brecha*, 5 de septiembre de 2014: 34-35.
- Gesteira, S. (2014): Más allá de la apropiación criminal de niños: el surgimiento de organizaciones de personas adoptadas que buscan su identidad biológica en Argentina. *Runa* (35)1: 61-76.
- Locke, J. (1975) [1694]. *An Essay Concerning Human Understanding*. P. Nidditch, Oxford: Clarendon Press.
- Mackie, D. (1999): Personal Identity and Dead People. *Philosophical Studies*, 95: 219-242.
- Noonan, H. (2003): *Personal Identity*. London: Routledge.
- Olson, E. (1997): *The Human Animal: Personal Identity Without Psychology*. Oxford: Oxford University Press.
- Parfit, D. (1971): Personal Identity. *Philosophical Review*, 80: 3-27.
- Thomson, J. J. (1997): People and Their Bodies. In: Dancy, J. (ed.): *Reading Parfit*, Oxford: Blackwell.
- Villalta, C. (2012): *Entregas y secuestros. El rol del Estado en la apropiación de niños*. Buenos Aires: Editores del Puerto/CELS.
- Williams, B. (1956-7): Personal Identity and Individuation. *Proceedings to the Aristotelian Society*, 57: 229-52.

Philosophy of science and inductive risk

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In recent years, the notion of inductive risk, or the idea that reasoning on the basis of empirical evidence is always accompanied by the risk of being mistaken, has enjoyed considerable attention of philosophers of science. The aim of my presentation is to argue that to the degree that philosophy of science uses empirical data in its arguments, philosophy of science itself has to face the problem of inductive risk. Using an example of an empirically based philosophical argument (Löhkivi et al 2012), I demonstrate how it is subject to inductive risk. While the authors of the argument do not address this issue directly, I show how non-epistemic values play a role in their argument, as many philosophers of science suggest they would in the situation of inductive risk. Finally, I offer some reflections on the possibilities to deal with values in a philosophical argument of this kind.

While the discussion over the formulation, and the implications, of the problem of inductive risk is ongoing (Elliott and Richards 2017b), the recognition of this issue has inspired active research in philosophy of science, from general arguments about the role of values in science (e. g., Douglas 2009) to studies of inductive risk in specific scientific disciplines (e. g., Elliott and Richards 2017a).

At the same time, and often in the work of the same philosophers, philosophy of science has been showing an increasing interest in empirically informed studies of science – analyses of specific disciplines, practices or laws and regulations relevant for research. Justin Biddle's (2014, 15) proposal to “begin with the actual organisation of research, in all of its messiness, and attempt to improve how research is organised in a piecemeal, iterative, and empirically-based manner” is a clear statement of this vision for philosophy of science.

I suggest that this growing prominence of empirically based reasoning in philosophy of science makes the notion of inductive risk applicable to such philosophical arguments – when making conclusions and proposing improvements on the basis of empirical information about social organisation or research, there is always the possibility of making an error.

In the presentation, I demonstrate the problem of inductive risk in a philosophical argument on the example of an analysis of research evaluation practices in humanities (Löhkivi et al 2012). The analysis builds on a number of interviews with researchers in Estonian academia to demonstrate that there is a mismatch between the type of work humanities researchers consider valuable and meaningful – for example, writing books in Estonian that contribute to the general cultural conversation – and the work that the official evaluation practices prioritise – English-language articles in high-impact international journals. The analysis then argues that these evaluation practices perpetuate epistemic injustice with respect to humanities, undermining their credibility (and their funding prospects).

I suggest that making this kind of claim on the basis of empirical material clearly carries with it the risk of being mistaken and accepting a false hypothesis or rejecting the true one – in other words, inductive risk.

Beginning with the classical paper on inductive risk by Richard Rudner (1953), philosophers of science have argued that dealing with inductive risk involves ethical judgements – seeing a specific type of error as more important to avoid reflects value judgements concerning the badness of different possible outcomes. I suggest that the analysis in question demonstrates such value judgements. For example, the value of humanities research can be seen as a consideration that makes underestimating the threat that unfair evaluation practices pose to humanities an especially worrisome possibility and

thus especially important to avoid. This, in turn, influences the strength of evidence that is considered sufficient for accepting the hypothesis that current evaluation practices commit epistemic injustice with respect to humanities.

While the analysis in question does discuss some relevant values, it does not frame the discussion in the terms of inductive risk and does not focus on justifying the values used. I conclude the presentation by reflecting whether some of the methods proposed by philosophers in order to address the issue of values in research in a fairer, open and trust promoting way (e. g., Douglas 2009) could be applicable when developing research in philosophy. For example, could philosophers in this case consult the full range of stakeholders in order to clarify their values? Who counts as a stakeholder in the case of an argument about the social organisation of science? How could such a discussion look like and how could its results be used?

I conclude that certain areas of philosophy of science pose interesting questions for, and deserve attention of, philosophers working on inductive risk.

References

- Biddle, J. B. (2014): Can Patents Prohibit Research? On the Social Epistemology of Patenting and Licensing in Science. *Studies in History and Philosophy of Science* Part A 45, 14–23.
- Douglas, H. E. *Science, Policy, and the Value-Free Ideal*. Pittsburgh: University of Pittsburgh Press.
- Elliott, K. C., Richards T. (eds)(2017a): *Exploring Inductive Risk: Case Studies of Values in Science*. New York: Oxford University Press.
- Elliott, K. C., Richards, T. (2017b): Exploring Inductive Risk: Future Questions. In: Elliott, K. C., Richards, T. (eds): *Exploring Inductive Risk: Case Studies of Values in Science*, 261–277. New York: Oxford University Press.
- Endla, L., Velbaum, K., Eigi, J. (2012): Epistemic Injustice in Research Evaluation: A Cultural Analysis of the Humanities and Physics in Estonia. *Studia Philosophica Estonica* 5(2), 108–132.
- Rudner, R. (1953): The Scientist Qua Scientist Makes Value Judgements. *Philosophy of Science* 20(1), 1–6.

Constituting frequency changes in genetic populations via approximation and stability: Understanding the role of the Hardy-Weinberg principle through its epistemic history

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Recent work in the HPS has focused on the peculiar epistemic function of some principles within certain systems of knowledge, especially in physical theories. These principles are said to ‘constitute’ a scientific framework, since they allow for the identification of the objects of scientific inquiry without being empirically testable in the same way as the other parts of that framework. One overarching perspective on constitutive principles is developed by Friedman (2001), who identifies ‘constitutivity’ with the function of certain theory-relative principles, such as the light principle in special relativity, which coordinate our mathematical representations with the empirical phenomena in space-time theories.

The biological sciences have received very little attention with respect to this philosophical focus. Nonetheless, population genetics is a good starting point to assess the fruitfulness of constitutive views of science beyond the physical sciences, given its relatively advanced stage of maturity and its high degree of mathematisation. To this aim, I analyse the history of the Hardy-

Weinberg principle (HWP), with the aid of the three parameters that I identify as the core features of the constitutive function: quasiaxiomaticity, generative potential, and empirical shielding. Firstly, by focusing on the epistemic role of the HWP during the early days of population genetics, I show how this principle, which captures the equilibrium state of genetic populations, was held fixed and practically unquestioned in the writings of the ‘fathers’ of population genetics (cf. Fisher 1918, 1922; Haldane 1924; Wright 1917, 1918), thus showing a high degree of quasi-axiomaticity, and even though it was not well-established or tested, it served as a foundation for much theoretical and empirical work.

Secondly, following Rheinberger (2013), I take the HWP to function as a counterfactual idealised situation, where any deviation from the ratios it expresses points to hidden interactions in need of explanation. Whereas its mathematical formulation does not allow any inferences to phenomena in and of itself, but – at most – it constrains which interpretations of the formalism are possible given the variables and their interaction (Millstein et al. 2009), the counterfactual character of the HWP comes to work when the formalism is used to interpret a frequency distribution and to make hypotheses on the deviations from expected frequencies. Therefore, I argue that the HWP has generative potential in virtue of its counterfactual character qua equilibrium state, which thus lies at the core of its constitutive role. However, since its formulation conceptually depends on the mathematics of probability and on the Mendelian scheme, it would at best count only as minimally constitutive, if understood on the lines of Friedman’s mathematical-physical coordinating principles. Nonetheless, I show how the counterfactual character of the HWP is amenable to another interpretation of the constitutive function, in terms of counterfactual reasoning (Buzzoni 2013; Stuart 2017).

Thirdly, I argue that – to fully understand the role of the HWP within population genetics – it is crucial to clarify its relationship with two more domain-general constitutive epistemic principles, which are at work when the HWP is deployed to construct empirical models and to make inferences about phenomena: approximation and stability. The formulation of the HWP introduces strong idealisations especially the requirement of infinite populations. It is in virtue of its highly idealised character that the HWP has a high degree of empirical shielding, which contributes to its constitutive character. Following Abrams (2006), and in line with the counterfactual character of the HWP, the requirement of infinite populations should not be taken as referring, but only as exhibiting boundary limits. Nonetheless, to allow the construction of more empirical models from the highly idealised equilibrium state, so that inferences to phenomena may be justified, it is necessary to assume the principle of approximation (Van Fraassen, 2008), which states that, if certain conditions follow from the ideal case (or model), then approximately these conditions will follow from an approximation to the ideal cases.

Finally, I spell out the connections between the HWP as equilibrium state and the notion of stability. Stability has been recently analysed as a necessary epistemic assumption required by our practices of representational modelling to justify inferences from models to phenomena (Fletcher, under review). However, here I rely on a different, albeit related, understanding of it as an epistemic condition on a class of phenomena under investigation. Scientific literature focusing on the limitations of the HWP (Li, 1988; Stark, 2006a, 2006b, 2007; Stark and Seneta, 2013) led to proposals of reconceptualising the equilibrium state of genetic population in terms of dynamical systems, whereby the HWP would only count as one stable state, but not as the equilibrium state (Bosco et al. 2012). By relating this research to discussions about ‘structural’ stability in dynamical systems theory (Guckenheimer and Holmes 1983), I show that, if the HWP is no longer able to capture the state at which a system is unchanging, inferences from models deploying the HWP are then justified only by the assumption of stability of (a certain class of) systems, where the epistemic status of this assumption must then be subject to further scrutiny.

References

- Abrams, M. (2006): Infinite populations and counterfactual frequencies in evolutionary theory. *Studies*

- in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, 37(2), 256–268.
- Bosco, F., Castro, D., and Briones, M. R. (2012): Neutral and stable equilibria of genetic systems and the Hardy–Weinberg principle: limitations of the chi-square test and advantages of auto-correlation functions of allele frequencies. *Frontiers in Genetics*, 3, 1–10.
- Buzzoni, M. (2013): On thought experiments and the Kantian a priori in the natural sciences: A reply to Yiftach J. H. Fehige. *Epistemologia*, 36, 277–293.
- Fisher, R. A. (1918): The correlation between relatives on the supposition of Mendelian inheritance. *Transactions of the Royal Society of Edinburgh*, 52, 399–433.
- Fisher, R. A. (1922): On the dominance ratio. *Proceedings of the Royal Society of Edinburgh*, 42, 321–341.
- Fletcher, S. C. (2017): *The principle of stability*. Manuscript submitted for publication.
- Friedman, M. (2001): *Dynamics of Reason*. Stanford, CA: CSLI Publications.
- Guckenheimer, J., Holmes, P. (1983): *Nonlinear Oscillations, Dynamical Systems, and Bifurcation of Vector Fields*. Springer, New York
- Haldane, J. B. S. (1924): Part I. A mathematical theory of natural and artificial selection. *Transactions of the Cambridge Philosophical Society*, 23, 19–41.
- Li, C. C. (1988): Pseudo-random mating populations. In celebration of the 80th anniversary of the Hardy–Weinberg law. *Genetics*, 119(3), 731–737.
- Millstein, R. L., Skipper Jr, R. A., and Dietrich, M. R. (2009): (Mis) interpreting mathematical models: drift as a physical process. *Philosophy & Theory in Biology*, 1.
- Rheinberger, H. J. (2013): Heredity in the twentieth century: some epistemological considerations. *Public Culture*, 30(3), 477–493.
- Stark, A. E. (2006a): Stages in the evolution of the Hardy–Weinberg law. *Genetics and Molecular Biology*, 29, 589–594.
- Stark, A. E. (2006b): A clarification of the Hardy–Weinberg law. *Genetics*, 174, 1695–1697.
- Stark, A. E. (2007): On extending the Hardy–Weinberg law. *Genetics and Molecular Biology*, 30, 664–666.
- Stark, A. E., Seneta, E. (2013): A reality check on Hardy–Weinberg. *Twin Research and Human Genetics*, 16(4), 782–789.
- Stuart, M. T. (2017): Imagination: A sine qua non of science. *Croatian Journal of Philosophy*, 49(17), 9–32.
- Van Fraassen, B. C. (2008): *Scientific Representation*. Oxford University Press.
- Wright, S. (1917): Color inheritance in mammals. 6. Cattle. *Journal of Heredity*, 8, 521–527.
- Wright, S. (1918): Color inheritance in mammals. 11. Man. *Journal of Heredity*, 9, 231–232.

Philosophy of Science in Russia: the St. Petersburg Philosophical Society (1897-1923)

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The St. Petersburg Philosophical Society was established in 1897. Its founders were not only well-known philosophers (A.I. Vvedensky, V.S. Soloviev, E.L. Radlov, N.G. Debolsky, M.I. Karinsky, M.V. Bezobrazova), but also outstanding scientists and scholars – physiologist V.M. Bekhterev, botanists I.P. Borodin and A.S. Famintsyn, historians S.F. Platonov and A.S. Lappo-Danilevsky, philologist S.A. Zhebelev, physicist O.D. Khvolson and others. Thus, from the very beginning, the society became a platform for discussions on various topical problems from different scientific fields as principle of

interdisciplinary was laid in the basis of the activity of this scientific society. The papers presented and discussed at its meetings were devoted to philosophy of science as well. At that time in Russia, this direction of philosophy was just emerging. But, in the first quarter of the 20th century, a few philosophical conceptions of science were created by Russian scientist.

This paper considers several speeches by the members of the St. Petersburg on the problems of philosophy of science. For example, paper by the author of works on the philosophy of social sciences B.A. Kistyakovsky Categories of necessity and justice in the study of social phenomena , which was presented at the society's meeting on December 16, 1899. The speaker believed that unconditional necessity in its content is identical with all-dimensionality and timeliness. That is, with meaning for every place and for all time. B.A. Kistyakovsky pointed out that over any social phenomenon, regardless of the investigation of the causal relationships that caused it, an ethical sentence can be expressed, depending on whether it leads to just results. It should be noted that BA. Kistiakovskiy was still a representative of Marxist philosophy (he retired from Marxism later in the first years of the 20th century), and therefore the report gave a very positive assessment of a number of important Marxist theses. K. Marx was given special merit in the application of new methods to the study of social phenomena. The discussion of the speech was very active.

One of the most out-standing Russian scientist physiologist, academician B.M. Bekhterev gave a speech Strictly objective method in the study of neuropsychic activity and its role in substantiating human reflexology in February 1917. The speaker noted that external or objective manifestations of mental activity, both in animals and humans, can be considered as higher reflexes that are acquired through exercise or education, therefore, develop during life under the influence of individual experience. Reflexology, as a scientific discipline, sets as its task the study of neuropsychic manifestations, as higher reflexes. Admitting the influence of subjective factors on the course of mental processes, B.M. Bekhterev stressed that the subjective factors themselves are either a consequence of an external cause or they are concluded in the past conditions of the individual, which should be viewed from an objective point of view. In this case, the questions are useful not to dwell on the subjective aspects of mental activity, but to take into account objective data. In conclusion, he noted that a strictly objective method of investigating neuropsychic activity had already indicated the complete pattern of development and manifestation of higher reflexes. Similarly, in public life, objective study has yielded many interesting data in studies of the laws of human action and the laws governing the development of a number of important social phenomena.

After the revolutionary events of 1917, the society temporarily stopped its activities. But after the end of the Civil War the society resumed its work and several papers on philosophy of science were presented at the meetings immediately. Papers by N.V. Boldyrev Contemplation and Reason, Being and Cognition (April 17 and 24, 1921), by A.V. Vasiliev On the history of the general principle of relativity (May 8, 1921), by S.A. Alekseev Analogy as the main method of cognition (May 29, 1921):

In conclusion, it should be stressed that philosophy of science was one of the important directions the activities of the St. Petersburg Philosophical Society during the entire period of its existence.

Nevertheless, in the framework of the Soviet power's struggle with dissent, the expulsion of many outstanding philosophers was made in the fall of 1922. Among the expelled were active members of the Philosophical Society. After a half of year the society was closed. Russian philosophy free from the ideology was destroyed.

Acknowledgements

The reported study was funded by RFBR according to the research project № 18-011-00730.

Examining the Structured Uses of Concepts as Tools: Converging Insights

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Examining the historical development of scientific concepts is an important step in understanding the structured routines within which these concepts are used as goal-directed tools in experiments. To illustrate this claim, I will outline how the concepts of mental imagery and hallucinations each inherited an interdependent set of associations from earlier uses that, although nominally-discarded, continues to structure their current independent uses for pursuing unrelated experimental goals. In doing so, I will highlight how three distinct accounts of conceptual practice offer mutually instructive insights for understanding how the uses of our current (momentarily) stable scientific concepts contribute to experimental practices.

As evident in my terminology, the first account is drawn from the recent scholarship examining how the uses of scientific concepts can enable scientific practices (e.g., Boon 2012; Brigandt 2012; Feest 2010; Steinle 2012): In this context, scientific concepts have been described as tools that function in ways that contribute to empirical knowledge; contributions that extend beyond their traditionally recognised roles in mental and linguistic representation. The second account can be glimpsed within the technoscientific studies focus on non-human agency that highlight how the disciplined routines within which concepts are used act in analogous ways to the routinized participation of machines in experiments (e.g., Pickering 1995, 29, 70): The third account I wish to highlight draws attention to how the functions of concepts are grounded by the set of historically-contingent experimental practices (e.g., Canguilhem 2008, 9, 43, 76):

Although maintaining important differences, these three approaches to analysing conceptual practice are mutually instructive for understanding the historically-contingent uses of our current (momentarily) stable scientific concepts.

Together, they offer a view of the uses of concepts as taken-for-granted tools that function within networks of subterranean associations; associations that are grounded by historically contingent experimental practices that structure how they act (in concert with human and material participants) within the temporally dynamic processes of scientific practice. To illustrate the value of this way of examining the uses of concepts, I will outline how the concepts of mental imagery and hallucinations each inherited an interdependent set of associations that, although nominally-discarded, continues to structure their uses for pursuing unrelated experimental goals.

By drawing together diverse insights for understanding the historically-contingent uses of our current (momentarily) stable scientific concepts, I seek to illustrate one way of highlighting that the independent uses of two scientific concepts can inherit an interdependent set of nominally-discarded associations that structures their uses in experimental practice.

References

- Andersen, H. (2012): Conceptual Development in Interdisciplinary Research. In: Feest, U., Steinle, F. (eds.): *Scientific Concepts and Investigative Practice*. 271–92. Berlin Studies in Knowledge Research, volume 3. Berlin: De Gruyter.
- Bloch-Mullins, C. L. (2015): Foundational Questions about Concepts: Context-Sensitivity and Embodiment. *Philosophy Compass* 10 (12):940–52. <https://doi.org/10.1111/phc3.12272>.
- Boon, M. (2012): Scientific Concepts in the Engineering Sciences: Epistemic Tools for Creating and Intervening with Phenomena'. In: Feest, U., Steinle, F. (eds.): *Scientific Concepts and Investigative Practice*. 219–44. Berlin Studies in Knowledge Research, volume 3. Berlin: De Gruyter.

- Brigandt, I. (2012): The Dynamics of Scientific Concepts. In: Feest, U., Steinle, F. (eds.): *Scientific Concepts and Investigative Practice*. 75–103. Berlin Studies in Knowledge Research, volume 3. Berlin: De Gruyter.
- Canguilhem, G. (2008). *Knowledge of Life*. Edited by Paola Marrati and Todd Meyers. Translated by Stefanos Geroulanos and Daniela Ginsburg. 1st ed. New York: Fordham University Press.
- Davidson, A. I. (2001): *The Emergence of Sexuality: Historical Epistemology and the Formation of Concepts*. Cambridge, Mass: Harvard University Press.
- Feest, U. (2010): Concepts as Tools in the Experimental Generation of Knowledge in Cognitive Neuropsychology. *Spontaneous Generations: A Journal for the History and Philosophy of Science* 4 (1):173–90. <https://doi.org/10.4245/sponge.v4i1.11938>.
- Machery, E. (2007): 100 Years of Psychology of Concepts: The Theoretical Notion of Concept and Its Operationalization. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 38 (1):63–84.
- Nersessian, N. J. (2008): *Creating Scientific Concepts*. Cambridge, MA: MIT Press.
- Pickering, A. (1995) *The Mangle of Practice: Time, Agency, and Science*. Chicago: University of Chicago Press.
- Pickering, A., Stephanides, A. (1992): Constructing Quaternions: On the Analysis of Conceptual Practice. In: Pickering, A. (ed.): *Science as Practice and Culture*. 139–67. Chicago: University of Chicago Press.
- Steinle, F. (2005): Experiment and Concept Formation. In: Hájek, P., Valdés-Villanueva, L. M., Westerståhl, D., (eds): *Logic, Methodology, and Philosophy of Science: Proceedings of the XII International Congress*. 521–36. Studies in Logic and the Foundations of Mathematics. London: King's College Publications.
- Steinle, F. (2012): Goals and Fates of Concepts: The Case of Magnetic Poles. . In: Feest, U., Steinle, F. (eds.): *Scientific Concepts and Investigative Practice*. 105–26. Berlin Studies in Knowledge Research, volume 3. Berlin: De Gruyter.

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Acknowledgements

The EENPS 2018 Bratislava conference is supported by Slovak grant scheme VEGA No. 1/0036/17

Argumentation in Science and Philosophy: Logical, Methodological, and Pragmatic Aspects .

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